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THE SPECTRUM OF COMET MOREHOUSE

By A. FOWLER

In a recent description and discussion of their admirable photographs of the spectrum of Comet Morehouse,[1] Messrs. Pluvinel and Baldet have confirmed the presence of the "low pressure" spectrum of carbon monoxide to which I had previously called attention.[2] The results of their work in this connection are summarized in the following words:

> The number of our doublets is 21, while Fowler's are only 12. Hence, there remain 9 doublets which have not been observed in the laboratory. Furthermore, of the 12 doublets, 2 do not agree very satisfactorily, as already explained. But the accordance of the two spectra in their brightest and most conspicuous parts is too close to admit of any doubt that the doublets in the spectrum of Comet Morehouse are practically identical with those of carbon monoxide at very low pressure.

Pending a more complete investigation of the laboratory spectrum, I should like to explain that in preparing my list of bands I was careful to include only such doublets as could be well established on the photographs available; some were doubtless overlooked on account of their faintness and admixture with other bands characteristic of carbon monoxide at higher pressures. The less refrangible parts of the spectrum in particular were incompletely recorded on this account, and most of the cometary doublets

[1] *Astrophysical Journal* **34**, 89, 1911.

[2] *Monthly Notices R. A. S.*, **70**, 176, 484, 1910.

not shown in my tables were situated in this region. The cometary spectrum, however, may be expected to aid in the proper identification of the bands in the terrestrial spectrum, as it is evident that in the tails of comets the peculiar spectrum in question is more completely isolated than in any laboratory spectrum which I have hitherto obtained. An increase in the relative intensity of the tail bands as the density of the gas is diminished is very definitely indicated by the observations which have been made, and the present difficulty is to produce sufficient luminosity when the gas is rarefied to such an extent that all other carbon monoxide bands might be expected to disappear. Further experiments, however, will be undertaken with special reference to the missing bands.

As to the discrepancies in the tabulated wave-lengths of the terrestrial and cometary bands, no claim for great accuracy could be made in my own work, as the spectrum was always feeble, and only small dispersion could be employed. Nevertheless, the errors in the main were unlikely to amount to more than an Ångström unit, except perhaps for the bands in the green, and I have gone over the measurements again without finding any substantial changes in the published wave-lengths to be necessary. In particular, I have confirmed the position of the band λ 3415, 3429, which Pluvinel and Baldet identify with their comet doublet 3436, 3446, and the discordance must therefore be attributed to the faintness of this part of the cometary spectrum, and the probable absence of suitable reference lines in this region of the stellar comparison spectrum which was used in the determination of wave-lengths. The other principal discrepancy to which attention is drawn is between the doublet of the laboratory spectrum at λ 4887, 4916, and that of the comet 4846, 4879; the former was noted by me only as "an indication of a faint band," and it should not have been identified with a cometary band so far distant. Considerations as to the arrangement of the bands in series, however, make it probable that the comet doublet λ 4846, 4879 really belongs to the carbon monoxide spectrum, as will appear later. The want of agreement in the wave-lengths, as a whole, is not greater than might be expected from the nature of the photographs of the

cometary spectrum given by a prismatic camera, and from the use of the spectrum of *Vega* as the only term of comparison.

The arrangement of the bands in series by Pluvinel and Baldet seems to call for further consideration. I wish to suggest that the less refrangible bands which they have included in the brighter (A) series should not be regarded as part of this series at all, but as forming a distinct series in themselves, probably related to the other series in the manner indicated by the work of Deslandres in the case of nitrogen. Taking my own values for the more refrangible components of the doublets of the A series (i.e., Pluvinel and Baldet's A_2) the positions are given very closely by the equation

$$n=65008-13\cdot5(m+0\cdot444)^2,$$

where n is the oscillation-frequency in air, and m has successive integer values ranging from 51 to 56. The resulting calculated values are compared with the observed wave-lengths in Table I.

TABLE I

m	λ calc.	λ obs.	O−C
56	4545.4	4545.4	0.0
55	4253.9	4253.2	−0.7
54	4001.3	4001.3	0.0
53	3781.0	3781.0	0.0
52	3587.0	3587.0	0.0
51	3415.3	3415.0	−0.3

The satisfactory agreement in the case of these six well-established bands suggests that the formula may be used to predict the approximate positions of any less refrangible bands which may form part of the same series. These work out at 4887, 5292, 5779, and 6375; but with the possible exception of the first, it is to be expected that they would be too faint for observation, and none of them agrees with Pluvinel and Baldet's measurements of the cometary bands.

A very similar result is obtained even if the probably less accurate wave-lengths given by Pluvinel and Baldet are made the basis of calculations. The bands corresponding to the first, third, and fifth, of Table I, give the equation

$$n=73392-11\cdot25(m+0\cdot6)^2.$$

The positions calculated from this equation are shown in the second column of Table II, which also indicates the observed *minus* computed wave-lengths, compared with those derived by Pluvinel and Baldet from their own formula.

TABLE II

m	λ calc.	λ obs. in Comet (P & B)	O−C	O−C (P & B)
67	4549.2	4549.2	0.0	+17.8
66	4256.8	4256.9	+ 0.1	+ 5.4
65	4003.4	4003.4	0.0	− 3.0
64	3781.6	3782.6	+ 1.0	− 7.4
63	3586.0	3586	0.0	−11.6
62	3412.3	3436	+24	+10.5
61	3257.0	3269	+12	− 1.5

The positions of probably fainter bands of the same series given by this formula are 4890, 5292, 5774, and 6362, which do not differ materially from those calculated from my own wave-lengths. The important point is, however, that the five best determined bands can be represented much more closely than by the use of Pluvinel and Baldet's formula, in which the constants are based in part on the positions of less refrangible bands which they have assumed to belong to the same series. These less refrangible bands, if they belong to carbon monoxide at all, would therefore seem to constitute a separate series.

The fainter (B) series of doublets is apparently related to the A series in the usual manner; namely, that on the frequency-scale, one of the series may be superposed on the other, at least approximately, by an appropriate displacement. In other words, the constant a in Deslandres' formula $n=a+bm^2$ is alone different for the two series. Thus, for the more refrangible components of the B series of doublets, using my own wave-lengths, the equation becomes

$$n=62822-13\cdot5(m+0\cdot444)^2.$$

The calculated and observed values are compared in Table III.

The errors are not greater than might be expected from the nature of the data on which the calculations are based. The formula predicts other possible doublets of the B series with their

more refrangible components about 5984 and 6614, but these would probably be relatively faint.

From analogy with other band spectra, it is possible to predict additional series of bands of the tail spectrum, but until more accurate measures become available for the known bands it would be unwise to attach undue importance to the results. The value of the constant a for the extra series which is of most immediate interest may be derived from the corresponding numbers for the A and B series thus: $a_1=65008$; $a_2=62822=65008-2186$; $a_3=62822-(2186-2\times13.5)=60663$. The assumption that the second difference between successive values of this constant is identical

TABLE III

m	λ calc.	λ obs.	O−C	Remarks
57	5472.3	5473	+0.7	
56	5047.4	5049	+1.6	
55	4690.0	4688.5	−1.5	
54	4384.8	......	......	Confused with other bands
53	4121.2	......	......	in laboratory spectrum
52	3892.3	3891	−1.3	Doubtfully observed in laboratory spectrum
51	3690.9	3693	+2.1	

with that in the A and B series is not necessarily true, but they are usually not very different, and no great errors are likely to be introduced by taking them to be equal only when considering a series which is adjacent to the two which have been observed. Hence a third series may be approximately represented by the equation

$$n=60663-13.5(m+0.444)^2.$$

Doublets having their more refrangible components in the neighborhood of λλ 6872, 6205, 5665, 5218, and 4843 are therefore possible. It is probably this series which accounts for the less refrangible cometary bands included by Pluvinel and Baldet in their A series, their computed positions for the corresponding bands being 6853, 6205, 5674, 5230, and 4854. There are no very great differences in the positions of these bands calculated by the two processes, but my own values have been obtained without sacrificing the accuracy with which the well-established bands in the blue

and violet may be represented by Deslandres' formula. The well-marked cometary band λ 4846 is also more accurately represented by the λ 4843 of my computation than by the λ 4854 given by Pluvinel and Baldet's equation. The accuracy of the comet measures is probably not sufficient to distinguish between the respective values for the remaining bands. From these considerations, however, it is extremely probable that the less refrangible bands of the comet's spectrum had the same origin as those in the blue and violet.

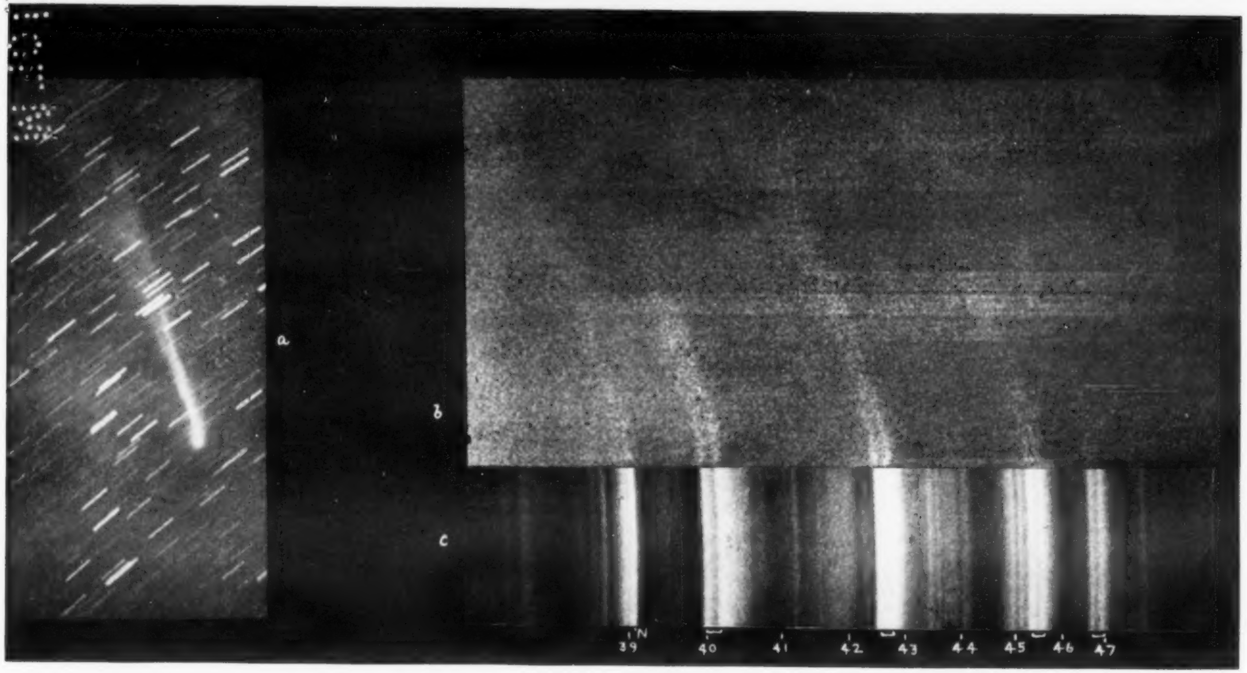
In what has gone before, no account has been taken of the less refrangible components of the doublets.[1] From my own measurements, and from analogy with certain other band spectra, there is every reason to believe that the intervals between the two components are constant throughout, if measured on the frequency-scale. Hence, the less refrangible components of the doublets should be represented by the same equations as the more refrangible ones if the separation of the components, amounting to about 118 on the frequency-scale, be subtracted from them.

If estimated in Ångström units, the intervals between the components of the doublets would vary as the squares of the wave-lengths, and would range from 14 Å at λ 3415 to 56 Å at λ 6872. Since the dispersion in the prismatic spectrum is nearly in inverse ratio to the cube of the wave-length, the separation on the photographic plates should be nearly inversely proportional to the wave-length. The doublet intervals observed in the comet spectrum by Pluvinel and Baldet, or computed by their formulae,[2] are of corresponding orders of magnitude for the bands on the more refrangible side of $H\beta$, but are considerably greater for the less refrangible bands. The discrepancies are probably to be attributed to the difficulty of obtaining accurate measurements in the case of the comet, but if Pluvinel and Baldet's formulae were correct, there should have been no difficulty in resolving the bands near 5218, 5665, and 6205, which their plates failed to show as doublets. My

[1] Professor Newall has found that, in the laboratory spectrum, each component of the doublets is itself a close pair, but this feature may be disregarded in the present discussion.

[2] The formulae are given in *Comptes Rendus*, **148**, 760–761, 1909.

PLATE IV



COMET MOREHOUSE (1908c) MARCH 20, 1908

a. Direct Photograph, 4 hours | *c.* Carbon Monoxide, pressure 0.01 mm
b. Objective Spectrogram, 7 hours } A. FOWLER, South Kensington
H. D. CURTIS, Santiago, Chile

own formulae, however, place the components closer together on the photographs than in the case of the bands in the blue, and therefore accord better with the comet observations; the formulae would not, however, admit of the comet bands 6848.4 and 7027.4 being regarded as the separate components of a doublet, as is assumed by Pluvinel and Baldet.

Although the identity of the cometary bands with those of low-pressure carbon monoxide may be considered as beyond question, it may be of interest to give the additional direct demonstration which is afforded by the photographs reproduced in Plate IV. For the photographs of the comet and its spectrum I am indebted to Dr. H. D. Curtis of the Lick Observatory, who has kindly placed enlargements on glass at my disposal. The photographs were taken at Santiago, Chile, on March 20, 1909, and the objective spectrograph employed had sufficient resolving power to show clearly the four principal bands of the comet as doublets.[1] The carbon monoxide spectrum is placed alongside that of the comet for easy comparison, and the identity of the two spectra will be evident at a glance. The strong band on the left in the terrestrial spectrum is the negative band of nitrogen 3914, due to an impurity in the carbon monoxide, and it will be seen that this is appropriately represented by a single band in the comet; other comets which have shown the carbon monoxide doublets, however, have not shown the nitrogen band.

It should be noted in conclusion that out of the five or six recent comets which have shown the low-pressure carbon monoxide spectrum, it was only in comet Morehouse that the bands were bright in the head as well as in the tail. The bands in question may therefore be regarded as especially characteristic of the tails of comets.

Imperial College of Science and Technology
South Kensington
December 6, 1911

[1] See Lick Observatory *Bulletin* No. 163.

"ON THE BEST VALUE OF THE SOLAR CONSTANT"

By C. G. Abbot and F. E. Fowle, Jr.[1]

It seems rather discouraging, after having spent ten of the best years of our lives on the determination of the solar constant of radiation, to be told[2] that we have relinquished reformed methods having a rational basis given them by Langley in favor of methods which do not differ essentially from those of Pouillet; that we have abandoned an essential principle recognized by many able investigators; that our value of the solar constant is to all intents and purposes the same as that of Pouillet, whose observations would yield our result if known corrections were applied for defects in the water pyrheliometer; that the admirable agreement of our separate measures and the small apparent probable error of our final result are but characteristics of the original Pouillet model, not to be regarded as evidences of the trustworthiness of our work; and finally that it is known that reliable measures of solar radiation may be made within the atmosphere which exceed the supposed value outside the atmosphere, as will be shown by Very in a subsequent paper.

We await with great interest the last item, namely: "reliable measures of solar radiation made within the atmosphere which exceed" 1.93 calories per square centimeter per minute. Our own pyrheliometers we thoroughly believe to be trustworthy instruments, for we have spent about seven years in establishing the standard scale of radiation. We have read our pyrheliometers in three successive years on Mount Whitney, altitude 4420 meters, the highest point in the United States, with exceptionally pure and dry air above us, and never yet have obtained readings exceeding 1.75 calories per square centimeter per minute. Had we used Ångström's pyrheliometer, adopted by the Solar Union as a standard instrument, our readings would have been 5 per cent lower.

[1] Published by permission of the Secretary of the Smithsonian Institution.

[2] F. W. Very, *Astrophysical Journal*, **34**, 371, 1911.

Ångström himself observed on Teneriffe,[1] at an altitude of 3683 meters, and never obtained values exceeding 1.63 calories.

To grasp the essential features of our determination of the solar constant of radiation the reader must consult: (1) the theory of our method given in *Annals of the Astrophysical Observatory*, Vol. II, pp. 13 to 17, 18 and 19; (2) the discussion of sources of error, pp. 58 to 82; (3) the procedure we pursue, pp. 50 to 57; (4) the measurements, pp. 83 to 98. Also, in recent years we have made very important improvements and measurements additional to those given in Vol. II of the *Annals*, as stated in this Journal, **29**, 281; **33**, 125; **33**, 191, and **34**, 197. Our work comprises spectrobolometric and pyrheliometric measurements made at high and low sun according to the method of Langley, as stated by him in his *Report of the Mount Whitney Expedition*, pp. 135 to 142, and Table 120, values 1 to 5. Our measurements have been made at Washington (sea-level), 1902 to 1907; Mount Wilson (1750 meters), 1905 to 1911; Mount Whitney (4420 meters), 1908, 1909, 1910; and at Bassour, Algeria (1160 meters), 1911. The total number of spectrobolometric solar-constant determinations now exceeds 600. All agree within narrow limits of variation to fix the value of the solar constant in terms of our standard water-flow pyrheliometric scale at about 1.93 calories per square centimeter per minute.

This value is supported to within the limits of his error of measurement by the determinations of Langley at Lone Pine and at Mount Whitney, where he obtained 2.06 and 2.22 calories respectively. We have proved (*Annals*, **2**, pp. 119 to 121) that the value 3 calories which Langley gave as the result of his Mount Whitney expedition is erroneous because of an error of logic which he committed in his discussion in the *Report of the Mount Whitney Expedition*, pp. 143 to 145.

This is a summary of the work against which Mr. Very has made the charges above mentioned. We should naturally expect that, having made such charges, he would have proceeded at once to attack one or more of the essentials of our work above enumerated, and have shown distinctly and numerically wherein, by errors of logic or of measurement, the work fails, and how large resulting errors have arisen.

[1] *Nova Acta R. Soc. Sci. Upsala*, 20, I, 1900.

But far from attacking our main work in this straightforward fashion he begins by misleading the reader to suppose that our value of the solar constant depends on raising to the 26th power some coefficients of water-vapor transmission obtained by Rubens and Aschkinass. If this matter formed any part of our definitive determination of the solar constant we should proceed to show how unfairly Mr. Very has used what we said about it. As it forms no part at all, we merely invite the reader to see what we did say from p. 168 clear through to p. 172 of Vol. II of the *Annals*, and then compare with what he says we said.

According to Mr. Very the reflection of *Mars* for total radiation is 0.27, that of *Venus* 0.92, and that of the earth "is more likely than not" to exceed the mean, or about 0.60. We have no confidence in any of these figures, and see no reason why a solar constant of 3 calories or upward must be conceded to allow them to stand. When he chose to blow hot instead of cold, Mr. Very attempted to show that *Neptune* could be maintained at 50° C. (!) by the solar radiation,[1] although the earth at less than 1/30 of *Neptune's* distance from the sun is only at 15° C. If planetary atmospheres can make such differences as this, we hardly think our careful direct measurements of the solar constant can be overthrown by computations made from the terrestrial temperature by the aid of guesswork about the reflecting and emitting power of the earth.

Mr. Very goes on to say that the temperature of the moon, as determined by him, requires a higher solar constant than 2 calories. We remember a very good story by Newcomb of a person who believed gravitation extended no farther than the atmosphere, and fell far short of the moon. Newcomb ascertained that his caller had never been to the moon to see, and told him that as he had never been there either, he doubted if they could agree! We feel some skepticism as to the temperature of the moon, and incline to praise the moderation of Langley, who, in summing up his classical investigation of it (in which Mr. Very assisted), says:[2]

[1] *Phil. Mag.* (6), **16**, 478, 1908.

[2] *National Academy of Sciences, Third Memoir*, Vol. IV, Part 2, p. 193; read 1887, published 1889.

The conclusion of the whole matter is, that we have been dealing with a subject almost on the limit of our power of investigation with the present means of science, and have reached no conclusion which we are absolutely sure of. . . . . If beyond this we can be said to be sure of anything it is that the actual temperature of the lunar soil is far lower than it is believed to be; but the evidence does not warrant us in fixing its maximum temperature more nearly than to say it is little above 0° Centigrade.

But Mr. Very admits no such uncertainty. For him the effective temperature of the moon's equatorial sunlit surface is 454° absolute, and nothing less than 3 calories will do for the solar constant to correspond. He maintains this, notwithstanding that Coblentz has shown that the moon is probably a very bad radiator![1]

Mars, according to Mr. Very, has a temperature too high to admit of a solar constant of 2 calories. We suppose he has no direct information as to the radiating power of that planet or its absorption of solar radiation. If we had not already had some experience of the unpleasantness of discussing *Mars*,[2] we should go on to say what we think about its temperature. We confess, however, that we know very little about *Mars*. Still, if *Neptune* can be maintained at 50° C. by solar radiation of about 1/1000 the intensity that reaches the earth,[3] we should think *Mars* might have some chance without a solar constant of 3 calories.

Mr. Very gives a meteorological argument which according to him shows that we have underestimated the loss of solar radiation in our atmosphere, and thus have derived too small a value of the solar constant. He gives an expression for the insolation of the earth, into which expression the average transmission of the atmosphere for solar radiation enters. He says the distribution of temperatures on the earth June 21 agrees with the requirements of his formula better when one takes the transmission at less than 0.25 than for higher values. He considers the best value 0.18 as the average transmission over the whole sunlit surface of the earth. For this he requires a solar constant exceeding 3 calories.

Mr. Very does not give sufficient details of this comparison to enable us to understand clearly what he has done. But at all

[1] *Physical Review*, **23**, 247, 1906.

[2] *Science*, **31**, 987, 1910.

[3] F. W. Very, *Phil. Mag.* (6), **16**, 478, 1908.

events we are of the opinion that the earth's surface temperature is a complicated function of many variables besides the insolation. Among these are cloudiness, distribution of land and water, mountains, ocean currents, and winds. Their effect is shown by the well-known differences of temperature at equal latitudes, as for instance between Europe and America. To choose that value of the atmospheric transmission which makes an insolation-curve match best with a temperature-curve, without any regard to these other factors, seems to us as indefensible as it would be to determine the reading of a Pouillet pyrheliometer merely by the rise of temperature on exposure to the sun, neglecting altogether the cooling due to the surroundings. We therefore can attach no weight at all to this method of fixing the solar constant.

In the remainder of this article we propose to take up the criticisms which Mr. Very makes of our work. He claims that we have abandoned Langley's methods and employ methods which do not differ essentially from those of Pouillet. *Pouillet observed only with the pyrheliometer.* We employ the pyrheliometer, as did Langley, only to determine the scale of energy of our spectro-bolometric determinations. Like Langley we determine the form of the solar energy-curve at different solar altitudes, correct it for instrumental absorption, determine numerous coefficients of atmospheric transmission from high- and low-sun observations on nearly homogeneous rays, determine thereby the form of the energy-curve outside the atmosphere, and assume, like Langley, that there is no water or oxygen absorption in the sun. We reduce the results to calories per square centimeter per minute as did he by the aid of the simultaneous readings of the pyrheliometer. Great improvements have been made in 30 years. We have the advantage of a better pyrheliometer than Langley, an automatic recording bolometer free from drift, and we can determine in 15 minutes all the data that it took Langley several successive days to obtain, and far more besides. We use many more atmospheric transmission coefficients than Langley did, and can determine them more accurately. We have made the measurements at sea-level, 1750 meters, and 4420 meters. In one thing only do we fail to follow Langley's example—we do not build a 2-calory solar constant up to 3 calories. We cite pp. 14 to

16 and 119 to 121 of Vol. II of the *Annals*, as against pp. 143 to 148 of Langley's *Report of the Mount Whitney Expedition*, as our defense for this course.

Mr. Very says Pouillet's result, if corrected for known errors in his pyrheliometry, will not differ essentially from ours. It will differ by about 10 per cent from ours, and for the reason that Pouillet made no spectrum observations.[1]

It will be a doctrine new to physicists that close agreement of independent determinations made under circumstances so diverse as those in our measurements at Washington, Mount Wilson, and Mount Whitney is no recommendation of the probable accuracy of the result; yet Mr. Very implies as much. As to his reflections on the effective radiating temperature of the earth and the transmission of water-vapor for long-wave rays, we think there is too much guesswork and too few facts, both on his side and ours, to make an argument about it worth while. After about five years more of experiments, we hope we may be in a condition to talk with him about these complicated questions without having to piece out our data with assumptions. We are surprised, however, by the roughshod manner in which Mr. Very overrides our value of the earth's reflection of total radiation. He merely remarks that he has "no hesitation in saying this is too small," without taking up the data by which we determined it. He suggests a mean between 0.27 and 0.92. We took more pains to obtain our value.

Mr. Very differs with us for our saying "we know that in general the lower layers of air have smaller transmission coefficients than the upper ones, owing to the generally low level of the larger quantities of dust and humidity." He says this "is true only for unsifted radiation and does not apply to the actual residual radiation which has already experienced its greatest absorption by aqueous vapor in the upper air." We reiterate our statement, both for Mr. Very's excepted kind of radiation and for homogeneous rays. If he still doubts it, let him look about in London, or look down on the dust layers above Pasadena from Mount Wilson. We should have thought his long residence near Pittsburgh would have convinced him that we are right. Certainly we still believe that light is more

[1] See Abbot's *The Sun*, Appleton & Co., 1911, pp. 293 to 296.

hindered near sea-level than in the higher atmospheric layers. If further verification is needed, see Table 118 of Langley's Mount Whitney Report.

Mr. Very objects to our process of evaluating the energy which would be found in the solar spectrum outside the atmosphere, beyond the wave-length where our observations stop in the infra-red. Perhaps he can suggest a better. At all events we hardly think he will claim that he can build up the solar constant to 3 calories out of energy beyond the wave-length 2.5 μ.

Mr. Very says we ought not to lay much weight on our observations made when the sun is near the zenith, and that we ought to observe at lower sun. He says we abandon low-sun observations "because low-lying mists render such measures uncertain" and prefer "midday observations on the plea that the sky is then clearer." He is quite wrong in both respects. We observe in the morning until about 10 A.M as a rule, and then stop because no decrease in air-mass worth waiting for occurs afterward, at the latitude of our stations in our observing season, May to November. We do not begin observing before the sun's altitude is 15° because, as we have shown (*Annals*, **2**, 63 to 64), the air-masses are uncertain at lower altitudes. We seldom observe when the sun is less than 40° or more than 75° from the zenith. We see no occasion to change our practice in these respects.

Mr. Very also regrets that we do not observe in winter. So do we, because we strongly suspect that the sun is a variable star. But if we did observe in winter we should require a *cloudless* region in or near the Southern Hemisphere, where the sun is *high* during our winter months. We see no advantage in extrapolating from air-mass 2 to air-mass zero when the extrapolation may begin at air-mass 1.2 just as well. However, there is no evidence from our winter observations at Washington that any systematic difference in the solar-constant values would arise on account of winter conditions.

Mr. Very intimates that even our narrow linear bolometer does not discern all the lines of atmospheric absorption, so that conditions may be conceived to exist under which our methods might give results appreciably too low. We have discussed this possi-

bility at considerable length elsewhere[1] and will not take space here to repeat that discussion in full. Such a condition would exist if what is sometimes called "the general absorption" of the atmosphere was in fact made up of nearly complete absorption and nearly complete transmission following one another in the spectrum in innumerable bands too narrow to be observed separately. We freely admit that the selective absorption of water-vapor in the great infra-red bands, and that of oxygen in its great bands, is of this character. Following the practice of Langley, we employ a special procedure for these bands, making the assumption, as he did, that none of them would exist in a spectrum taken at the outer limit of the atmosphere. We therefore draw a smooth curve for the energy spectrum outside the atmosphere where the terrestrial bands appear. This gives the highest possible solar-constant value. But as regards other regions of the spectrum, Rayleigh has shown satisfactorily that the so-called "general absorption" is really probably a scattering effect of small particles and molecules in the air, plus a diffuse reflection by larger particles. Schuster has shown that our atmospheric-transmission coefficients determined on Mount Wilson are almost exactly what could be predicted by Rayleigh's theory of the scattering of light by the molecules of air. Diffuse reflection and scattering is, according to Rayleigh, a continuous function of the wave-lengths. Hence our linear bolometer amply suffices to estimate the atmospheric transmission in regions where selective atmospheric absorption does not exist. But Mr. Very would have us admit an atmospheric band at $0.40\,\mu$ to $0.46\,\mu$. Our own work gives no certain intimation of this.[2] We cannot, however, see how an inspection of the extra-atmospheric energy-curve we have determined[3] could allow anybody to believe that any appreciable increase of the solar constant could come from smoothing the curve from $0.40\,\mu$ to $0.46\,\mu$, as if there were an atmospheric band there, in the manner we employ for the water-vapor bands of the infra-red.

[1] See *Annals of the Astrophysical Observatory of the Smithsonian Institution*, **2**, 64–65.

[2] *Ibid.*, Plate XVII.

[3] *Astrophysical Journal*, **34**, 206, 1911.

CONCLUSION

Mr. Very evolves a value of the solar constant of radiation from such unknown or fragmentary data as the reflection and emission of the earth, moon, and *Mars*, the temperatures of the two latter, and the dependence of terrestrial temperature on insolation. To clear the way for this he accuses us of doing what we have not, misrepresents what we have done, and suggests certain sources of error in our methods which we had already quantitatively discussed. We base our value on ten years of painstaking work, both theoretical and experimental, in laboratory and afield, at sea-level and high altitudes, comprising over 600 independent determinations by the most approved method.

Astrophysical Observatory
Smithsonian Institution
Washington, D.C.
January 1912

THE STARK-DOPPLER EFFECT FOR HYDROGEN CANAL RAYS IN AIR

By GORDON S. FULCHER

It has been found that light is produced by the collision of canal rays with gas molecules.[1] An attempt was made to explain the details of the Stark-Doppler effect, assuming that the collisions of canal rays with gas molecules obey the laws of ordinary, perfectly elastic impact. This was found to involve two other rather improbable assumptions: namely, that neutral canal rays do not cause the emission of an appreciable amount of light; and that it is the hit molecules, not the hitting, which emit the light showing the Stark effect. Also the fact that α particles are but slightly scattered in ionizing over 10^5 air molecules each, suggested that the assumption of perfectly elastic collision was wrong. The following experiment was devised to test this.

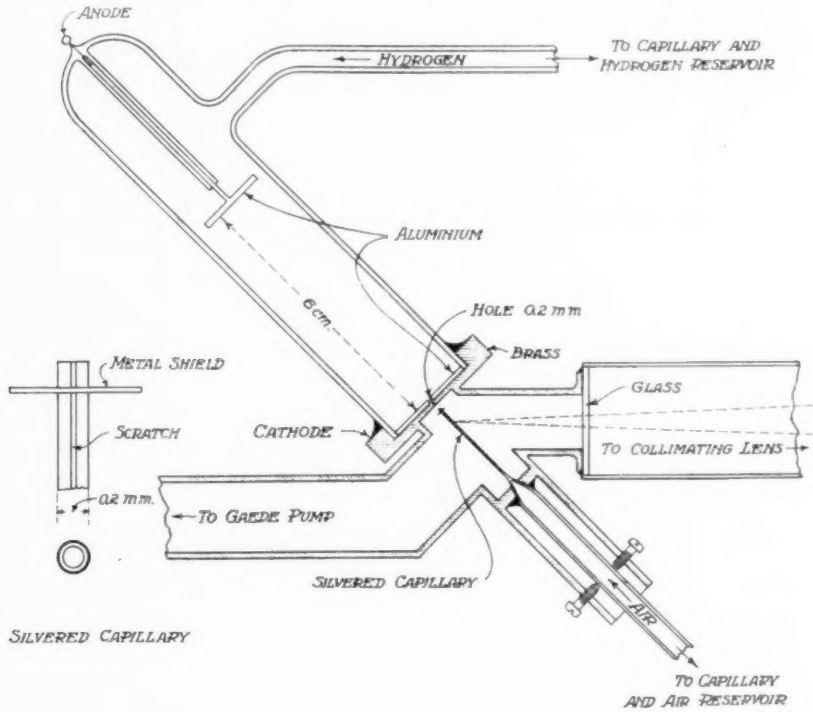
APPARATUS

The apparatus was arranged so that a fine beam of canal rays, after passing through a 0.2 mm hole in the cathode, was projected axially into a capillary about 0.3 mm in diameter. Hydrogen was supplied continuously to the discharge chamber through a second capillary, and air could be sent through the main capillary back of the cathode at a rate regulated by a third capillary and gas reservoir. The gases mixed in the chamber back of the cathode and were pumped away continuously by a Gaede pump. The stream of air was sufficient to prevent any appreciable quantity of hydrogen backing up into the capillary. Also very little air got into the discharge chamber, though that was much less important. In the main capillary, then, the only possible sources of the hydrogen lines were the hydrogen canal rays, luminescent as a result of their collisions with air molecules. It was necessary only to examine the hydrogen lines for the Stark effect to determine whether or not the hydrogen rays were stopped by their collisions

[1] G. Fulcher, *Astrophysical Journal*, **33**, 28, 1911.

with air molecules—whether they retained their energy, or handed it on to the hit molecules as previously assumed.

The main capillary was first silvered and the coating scratched off along a line parallel to its axis. Then it was set in place and adjusted to its proper position by means of three screws. The



APPARATUS FOR OBSERVING THE STARK EFFECT WHEN HYDROGEN CANAL RAYS BOMBARD AIR MOLECULES.

FIG. 1

scratch served as the slit of a spectrograph, which was placed so that the axis of the collimator made an angle of about 45° with that of the capillary. Two prisms were used; one, whose angle was 45°, was placed so that the angle of incidence was about 75°, considerably greater than for minimum deviation; the other, a 60° prism, was placed for minimum deviation. With this arrangement, the width of the lines was less than half the width of the

PLATE V

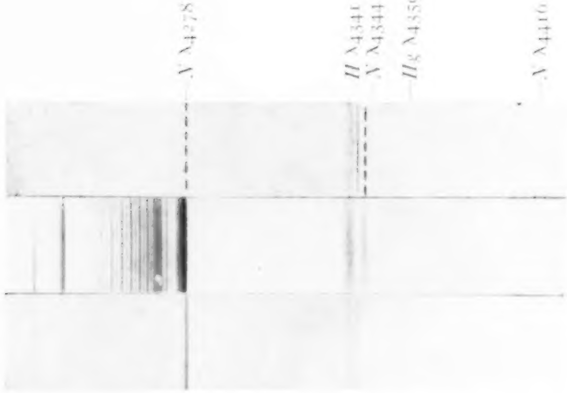


FIG. 2

HYDROGEN CANAL RAYS BOMBARDING:

(a) Hydrogen molecules; (b) Nitrogen molecules; (c) H and N molecules

FIG. 3

No. 162. *N* canal rays in *H*. 4500 volts, 15 hours

No. 160. *H* canal rays in *H*. 4000 volts, 1.5 hours

No. 167. *H* canal rays in air. 5000 volts, 24 hours

slit. A curious fact noticed is that if the order is reversed so that the 60° prism comes first, the dispersion is considerably less for the two prisms than for the 60° prism alone. Lenses of 45 cm and 55 cm focal length were used. The dispersion at λ 4300 was about 15 Å per mm. The depth of focus obtained is remarkable. Lines one cm long on the plates are of practically uniform width, though the slit, as is stated above, was inclined at an angle of 45° to the collimator. The light was so feeble that exposures of from 10 to 24 hours were necessary.

RESULTS

Parts of three of the first spectrograms obtained with the above apparatus are shown enlarged about five times in Fig. 2, Plate V. Some spectrograms obtained more recently are shown enlarged about twice in Fig. 3, Plate V. When hydrogen canal rays bombard hydrogen molecules, the series lines $H\beta$ and $H\gamma$ show the Stark effect very clearly. The "rest line" is seen to be distinctly separated from the broad "displaced line" by an "intensity minimum" (No. 160). When, however, the hydrogen canal rays bombard air molecules, the *displaced line alone is obtained*. No. 167 is interesting for another reason also: though it represents an exposure of 24 hours and the series lines of hydrogen are quite dense, it shows not a trace of the compound lines. If present, they must be relatively very much weaker than in the ordinary canal ray spectrum (No. 160). The cathode fall of potential was in all cases between 4000 and 5000 volts, and a continuous current was obtained by the use of a condenser and high resistance as in previous experiments.

These results mean that all the sources of the hydrogen lines under these circumstances have a considerable velocity, between 2×10^7 and 5×10^7 cm per second. What conclusions can we draw from this fact? Conceivably the emission of light by the canal rays may be due (1) to their collisions with air molecules, or (2) to their collisions with free electrons produced in connection with the ionization of the air by the canal rays. There are no other alternatives since the intensity of the light has been shown to depend directly on the pressure of the gas through which the rays are

passing (*loc. cit.*). Collisions with air molecules must occur of course, since the gas becomes ionized and the canal rays are stopped within a certain range. Let us first suppose that these collisions are perfectly elastic. The hit molecules will be given a considerable momentum, and all the light they emit as a result of the collisions will show a Doppler effect; the hitting rays, however, will emit most light when stopped or reflected back by the collision, and the light so emitted will show a broadened rest line, but no shifted line. But neither a Doppler effect for the nitrogen bands nor a rest line for the hydrogen lines was obtained in the above circumstances. Either, then, the collisions with air molecules do not result in an appreciable emission of light, or the collisions are not perfectly elastic.

Now consider the possibility of collisions with electrons. With ordinary discharge tube pressures, the ratio of the number of electrons present to the number of air molecules is less than one in a million. The chances of collisions with electrons are of course relatively much greater because of the electric field surrounding the charged rays. The deflection experiments with crossed magnetic fields made by J. J. Thomson,[1] and Koenigsberger[2] and his co-workers, have shown that at very low pressures such collisions predominate, perhaps, over molecular collisions. Such is certainly not true at the pressures used here (a few tenths of a millimeter). Also the impulse involved in such a collision would be over a thousand times less than in the case of a molecular collision. The effect would be the same as when hydrogen ions are neutralized by slow cathode rays whose energy is due to a potential difference of less than a volt. Such slow rays cannot produce ionization and in case of such recombinations would not cause sufficient disturbance to produce any appreciable luminescence.

It seems very probable, then, that canal rays emit light chiefly as a result of collision with gas molecules; and the fact that the light so emitted shows no rest line seems to prove that such collisions are not perfectly elastic, but that the rays retain a large proportion of their momentum after the collisions and do not impart much momentum to the hit molecules. The collisions then seem

[1] *Phil. Mag.*, **18**, 825, 1909.

[2] *Physikalische Zeitschrift*, **11**, 666–668, 1910; *Verhandlungen der deutschen physikalischen Gesellschaft*, **12**, 995–1017, 1910.

intermediate in type between the perfectly elastic collisions of ordinary gas molecules and the almost perfectly inelastic collisions of a rays with gas molecules.

Geiger[1] has determined the most probable angle through which an a particle is deflected by passing through certain thin metal foils, and also the way this angle varies both with the velocity of the a ray and the molecular weight of the metal causing the scattering. From these data he has computed the most probable angle of deflection resulting from collision with a single gold molecule. This involved certain assumptions, and the exact meaning of the word collision was not stated; but the results are very interesting, and accurate enough for our purposes. Extrapolating from his data down to lower velocities and smaller atomic weights, we find that when a helium canal ray with a velocity of 10^8 cm per second hits an air molecule, we may expect a most probable deflection of about 5°. For a velocity half as great the most probable angle should be about 40°. We may expect that hydrogen canal rays would be less deflected, in agreement with the conclusion stated above. It would be very interesting to determine the constants of scattering for canal rays of various kinds and velocities, and thus get deeper insight into the structure and internal electric fields of molecules. Only when this has been done will a quantitative explanation of the details of the Stark effect be possible.

The loss of energy resulting from the collisions may account for the fact that the maximum velocity of the sources of the shifted lines is always less than that computed from the cathode fall of potential.[2] The existence of the intensity minimum seems to show that the intensity of the light emitted as a result of any collision depends on the energy of impact, which will on the average vary directly with the energy of the bombarding rays,[3] falling to zero perhaps at a certain velocity depending on the molecules involved. It would be interesting to determine how the width of the intensity minimum depends on the kind of gas molecules bombarded. That it varies greatly is suggested by Strasser's results

[1] *Proc. Roy. Soc.*, (A) **83**, 492–504, 1910.

[2] F. Paschen, *Annalen der Physik*, **23**, 257, 1907; J. Stark, *Physikalische Zeitschrift*, **8**, 399, 1907.

[3] G. Fulcher, *Astrophysical Journal*, **33**, 40, 1911.

with mixed gases.[1] Here seems to be another example. Stark[2] reports that the Doppler effect in mercury is clearly obtained only with high potential differences, above 40,000 volts. Yet with mercury canal rays bombarding hydrogen molecules, I obtained a shift of 0.5 Å for $\lambda\lambda$ 4359 and 4047 with a cathode fall of less than 5000 volts (see below).

Reichenheim[3] found that in the spectrum of light from the path of anode rays, some strontium and calcium lines showed the Stark effect without any rest lines. The conditions were in fact quite similar to those obtained with the above apparatus; light was probably produced mainly as a result of the collision of metallic anode rays with molecules of iodine or some other electro-negative gas. His results tend to show that strontium, calcium, and probably other metallic anode rays with high velocities may retain a large part of their momentum after colliding with iodine or other gas molecules.

When nitrogen canal rays (5000 volts) bombard hydrogen molecules the negative bands alone are produced, and these show a Doppler effect (No. 162). The following are measurements made on two spectrograms, (*a*) *H* canal rays in *H* (trace of air) (No. 160), (*b*) *N* canal rays in *H* (No. 162).

Lines	(*a*) No. 160—in *H* mm	(*b*) No. 162—in *N* mm	(*a*) − (*b*) mm
H_{II} 4205.26	27.196	27.194	+ .002
H_{II} 4212.67	27.725	27.724	+ .001
Ca 4226.9	28.740	28.741	− .001
N−4256.2	30.775	*30.715	+ .060
N−4259.4	30.995	*30.938	+ .057
N−4262.3	31.184	*31.122	+ .062
N−4265.0	31.370	*31.314	+ .056
N−4278.0	32.243	*32.185	+ .058
H_γ shift line	*35.92 *36.12	———	———
H_γ 4340.6	36.220	36.212	+ .008
Hg 4358.6	*37.274	37.304	− .030
H_{II} 4412.42	40.452	40.457	− .005

* The starred lines are shifted.

[1] *Annalen der Physik*, **31**, 890–918, 1910.

[2] *Astrophysical Journal*, **25**, 180, 1907; *Annalen der Physik*, **21**, 439, 1906.

[3] *Annalen der Physik*, **33**, 747–761, 1910.

Each measurement given is the mean of five; the mean deviation from the mean is less than .002 mm, yet irregularities in the film cause a somewhat greater uncertainty. The width of the lines is .06 mm. The maximum shift for the $H\gamma$ line, λ 4340.6, is .30 mm (*a*); the mean shift for the *N* negative band, λ 4278, is .059 mm (*b*); while the mercury line λ 4358.6 is shifted .030 mm (*a*). The mercury line λ 4046.8 was also shifted on No. 160 a corresponding amount. These shifts were verified on other spectrograms. No. 160 was made with the discharge chamber in direct connection with the pump so that it contained mercury vapor from the pump, whereas in the case of No. 162 the only mercury vapor present was that which diffused over with the gases from the manometers connected to the gas reservoirs. Multiplying these shifts by the square roots of the atomic weights, $1, \sqrt{14}, \sqrt{200}$, we get 0.300, 0.223, and 0.423. Taking instead, $1, \sqrt{28}$, and $\sqrt{100}$, we get 0.300, 0.312, and 0.300. The shifts point to sources having velocities corresponding to singly charged nitrogen molecular rays and doubly charged mercury atomic rays. The cathode fall of potential was between 4000 and 5000 volts.

In the case of canal rays in air or pure nitrogen, both Stark[1] and Hermann[2] state that the nitrogen bands do not show a Doppler effect. There are two things to consider: First, the number of molecular canal rays compared to the number of atomic rays decreases as the voltage increases. I have never obtained any trace of any nitrogen spark lines with voltages less than 5000, whereas at higher discharge potentials the spark lines appear. The Doppler effect for the bands is then weaker for higher voltages and may be masked by the intense negative bands emitted as a result of the ionization of nitrogen molecules by secondary cathode rays or electrons produced by the canal rays.[3] Secondly, the scattering may be so much greater in the case of collision with nitrogen molecules than with hydrogen molecules that in the first case only a broadening of the lines results, whereas in the second a distinctly shifted line is obtained.

[1] *Göttingen Nachrichten*, p. 463, 1905; *Physikalische Zeitschrift*, **6**, 894, 1905.

[2] *Physikalische Zeitschrift*, **7**, 568, 1906.

[3] Cf. G. Fulcher, *Astrophysical Journal*, **34**, 388, 1911.

No trace of any positive bands could be found on any of the spectrograms (air into H), though long exposures were made with less dispersion. They are, then, relatively much weaker in comparison with the negative bands than in the ordinary canal ray spectrum. The negative bands of nitrogen, then, are emitted by the nitrogen molecules when ionized by cathode rays,[1] and also as a result of collision with hydrogen molecules.

UNIVERSITY OF WISCONSIN
February 1, 1912

[1] Cf. G. Fulcher, *Astrophysical Journal*, **34**, 388, 1911.

SPECTROSCOPIC STUDIES ON HYDROGEN

By HARVEY BRACE LEMON

Introduction.—The question as to whether any analogy exists between the behavior of line spectra and of continuous spectra, and if so, what are its limitations, has long been a mooted one in spectroscopy. For continuous spectra, excited solely by temperature, we have had a firm theoretical basis developed by Boltzmann, Wien, Planck, and others, upon which to work. Experiment has in a remarkable manner confirmed the deductions of theory. In the case of line spectra of gases and vapors, however, the situation is far otherwise. Until we have a more definite picture of what the emitting mechanism is, theory has no foundation upon which to build. To picture the mechanism we must have first of all agreement as to the phenomena it produces. As yet we have not even succeeded in this very preliminary requisite. We are not agreed upon the data at hand, still less are we prepared for their interpretation. With the large number of variables associated in the production of spectra of this type, it is not surprising that our data are as yet fragmentary and deal with very special cases. The importance of correlating these, however, in the hope that they may fit together and give us some clue to the mechanism beneath can not be overestimated. It may be that spectroscopic data alone will be insufficient—that perhaps a study of the ionization processes taking place simultaneously with the production of light may be necessary. The general aim of the present work will be confined to an attempt to bring the spectroscopic data available into concordance.

Because of the apparent simplicity of at least one portion of its spectrum and on account of its predominant character in the spectra of many stars, hydrogen has been one of the most widely studied elements, especially as to the question of analogy between the behavior of its series spectrum and that of continuous spectra. Kayser[1] and Langenbach[2] in 1903 attempting this parallel between

[1] "Zur Temperaturbestimmung strahlender Gase," *Boltzmann Festschrift*, p. 38 (July 1903), Barth, Leipzig, 1904.

[2] "Ueber Intensitätsverteilung in Linienspectren," *Annalen der Physik*, **10**, 789, 1903.

continuous and discontinuous spectra believed they detected a shift in the energy maximum of emission toward the violet with increased electrical excitation. The order of magnitude of this effect was considerable. If the same effect had been observed in the temperature radiation of a black body it would have corresponded to a rise in temperature of the radiator from 2200 to 2760 degrees absolute. This does not say, however, that such was the temperature of the hydrogen. In fact, looked at from other angles there seems to be little ground for any analogy whatever, for the comparison is between a so-called "luminescent" radiation excited solely by an electrical discharge and one whose origin lies in temperature alone. Kayser's and Langenbach's results have meaning only as showing a relation between the radiation and the intensity of excitation. Moreover, Nutting and Tugman[1] studying the effect of electrical conditions, primarily current and pressure, upon the hydrogen radiation, reach conclusions not in accordance with those of Kayser and Langenbach. Jungjohann[2] on the other hand agrees qualitatively with Langenbach as to the effect of pressure on the energy-maximum shift but disagrees with Kayser as to the correspondence of this shift with that of a black body. All of these experiments have been carefully conducted, but the methods used have been widely divergent, especially as to the mode of excitation employed; and to this and to one other cause about to be mentioned must the disagreements be ascribed.

Continuous background.—In making photometric measures on the spectrum of hydrogen one very important factor seems to have been universally overlooked, and it may be the neglect of this factor, more than experimental differences in method, has been the greatest cause of the discrepancies among workers in this field. This factor is the continuous spectrum of hydrogen. There are present in all ordinary forms of vacuum tubes filled with pure hydrogen three spectra—the series line spectrum, the compound line spectrum, and the continuous spectrum. The last, very faint

[1] "The Intensities of Some Hydrogen, Argon, and Helium Lines in Relation to Current and Pressure," *Bulletin Bureau of Standards*, **7**, 49, 1911.

[2] "Ueber Emission und Absorption leuchtender Gase bei hohen Stromdichten unter Verwendung von Gleichstrom," *Zeit. f. Wiss. Photog.*, **9**, 84, 105, 141, 1910.

in the red, becomes, even at low pressures (1–3 mm), quite strong in the blue and very marked in the violet. Obviously when examined under the low dispersion of a spectrophotometer it doubtless consists of a mixture of true continuous background and unresolved compound line spectrum. In any case measures made on the series lines $H\beta$, $H\gamma$, and $H\delta$, etc., will be largely in error unless correction is made for this continuous background on which these lines appear. A glance at the following table will show clearly the magnitude of this correction.

TABLE I

MAGNITUDE OF THE BACKGROUND CORRECTION

	α	β	γ	a	b	α_g	β_g	γ_g
Uncorrected	.084	.130	.169	.033	.094	....	....	....
Corrected	.084	.081	.035	.033	.045	.000	.049	.134

The figures represent the photometric intensities of the series lines $H\alpha$, $H\beta$, $H\gamma$, and of a group of secondary lines, a, in the red ($\lambda = .600–.603\ \mu$), b, in the blue ($\lambda = .492–.495\ \mu$), relative to the intensity of the corresponding portions of the spectrum of a Nernst glower. The gas was pure, under a pressure of .318 cm and was carrying a current of 6 milliamperes; the form of tubes and the apparatus is described fully below. The first line in the table gives the intensities as measured without any consideration being taken of the background. The second line shows that whereas the red lines α and a are unchanged, owing to the absence of the background in the red, the blue lines have their intensity values changed by 40 to 50 per cent and the violet line by as much as 80 per cent. The intensity of the background immediately adjacent to the red side of the series lines, denoted in the table by α_g, β_g, γ_g, is seen to rise very rapidly from zero in the red to a magnitude almost equal to that of γ in the violet. At lower pressures the background is not so strong, at higher pressures it is much more marked, in fact, it becomes so strong that $H\gamma$ is seen with difficulty, if at all, on it. That so large a source of error in the intensities of the hydrogen lines should have been overlooked by previous workers seems very strange. However, none of them in any place makes mention of

observations on the continuous background or of any correction of the line intensities due to it. Plate VI, a reproduction of photographs of the spectra given by some of the hydrogen tubes used, shows clearly the magnitude of the continuous ground. *A* is taken with the large dispersion of a 2-meter focus, $3\frac{1}{4}$-inch concave grating ruled by Professor Michelson; *B* is taken with a small prism for the dispersion piece, and shows the effect of pressure in strengthening the background.

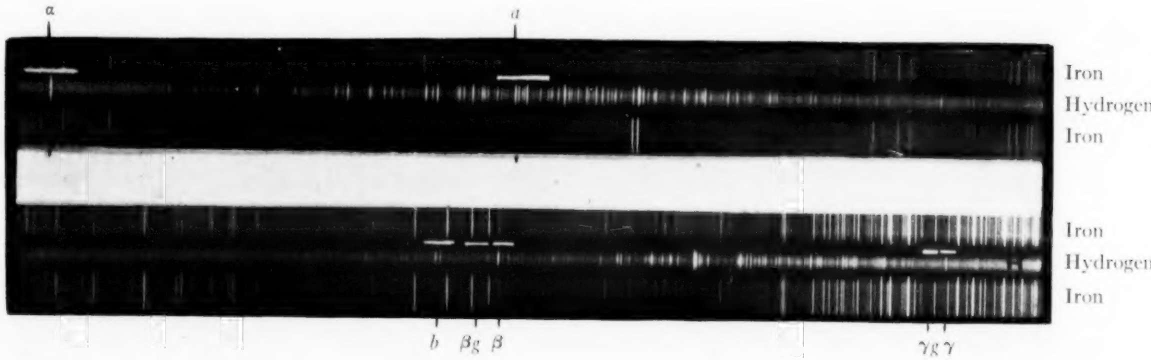
Outline of experiments.—The original aim of these experiments was an attempt to throw some light upon the open question as to the effect of temperature alone on the emission of hydrogen. The method was the very simple one of changing the temperature of the discharge tube through a large range and making photometric measures on the intensities of the various portions of the spectrum. Electrical conditions were of course to be held as nearly constant throughout as was possible. In the course of these first experiments there came to light the above-mentioned possible cause of the disagreement of previous workers on the question of the effect of electrical conditions, and the original aim of the undertaking has been broadened to a somewhat more extensive one. This will include the study of the effect of electrical conditions on the radiation of hydrogen, helium, and perhaps other elementary gases, as well as the effect of temperature alone.

PART I*a*

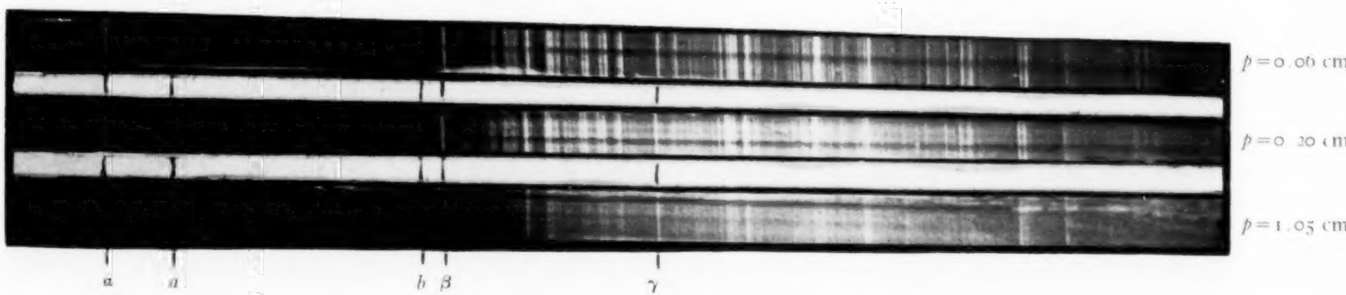
THE EFFECT OF TEMPERATURE UPON THE HYDROGEN SPECTRUM AS PRODUCED BY ALTERNATING CURRENT

Apparatus.—The apparatus consisted of the hydrogen generator and purifier, the discharge tubes, the electrical exciting equipment, and the spectrophotometer. A general view is given in the photograph, Plate VII. Fig. 1 is a detail drawing of the hydrogen generating and purifying apparatus. It was made entirely of glass with a minimum number of stopcocks. The gas was generated by the electrolysis of a dilute solution of orthophosphoric acid in tube *A*. So prepared, it contains a trace of oxygen which is absorbed in tubes *B* and *C*, each containing an alkaline pyrogallol solution.

PLATE VI



A

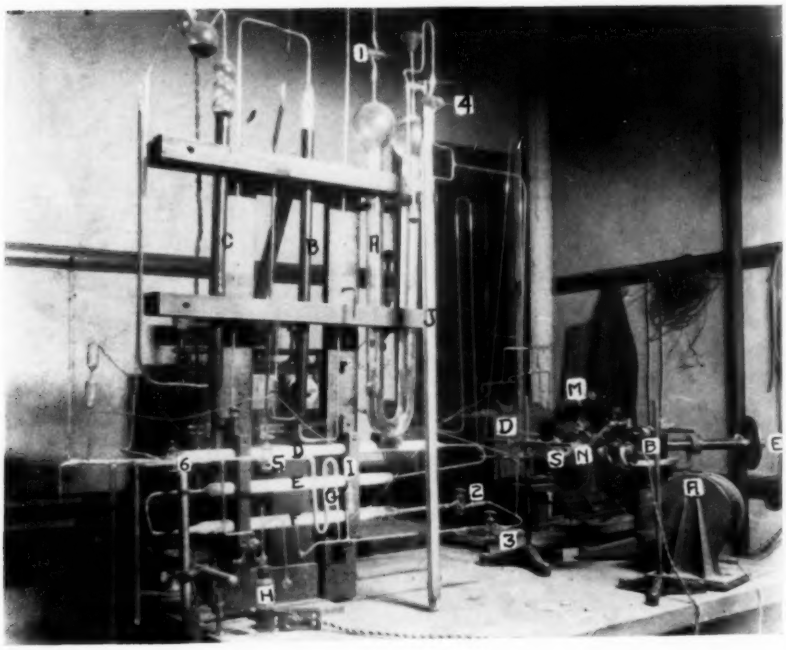


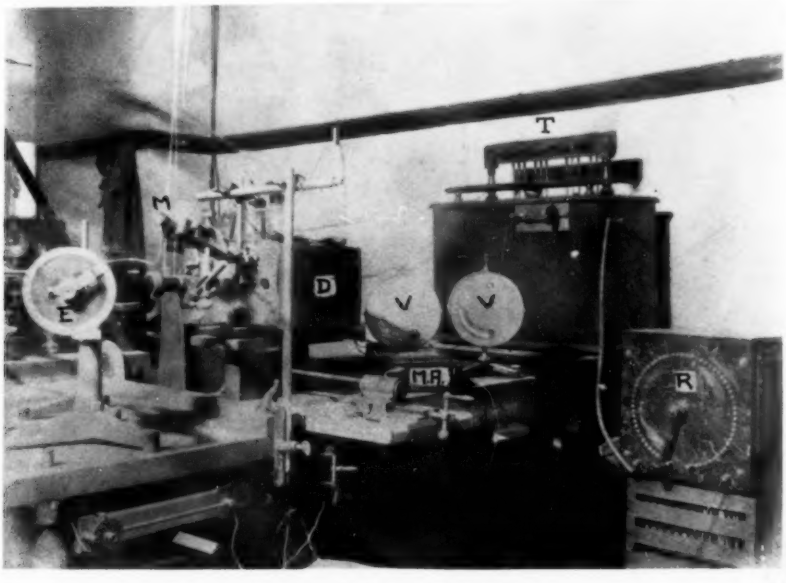
B

Spectrum of Hydrogen Tubes

A. With concave grating, iron comparison spectrum
B. With small prism, at different pressures

PLATE VII





General View of Apparatus

The formula due to Hempel was used, as this seems to be one of the few satisfactory solutions for completely taking up oxygen without giving off carbon monoxide. It consists of 15 gms pyrogallol in

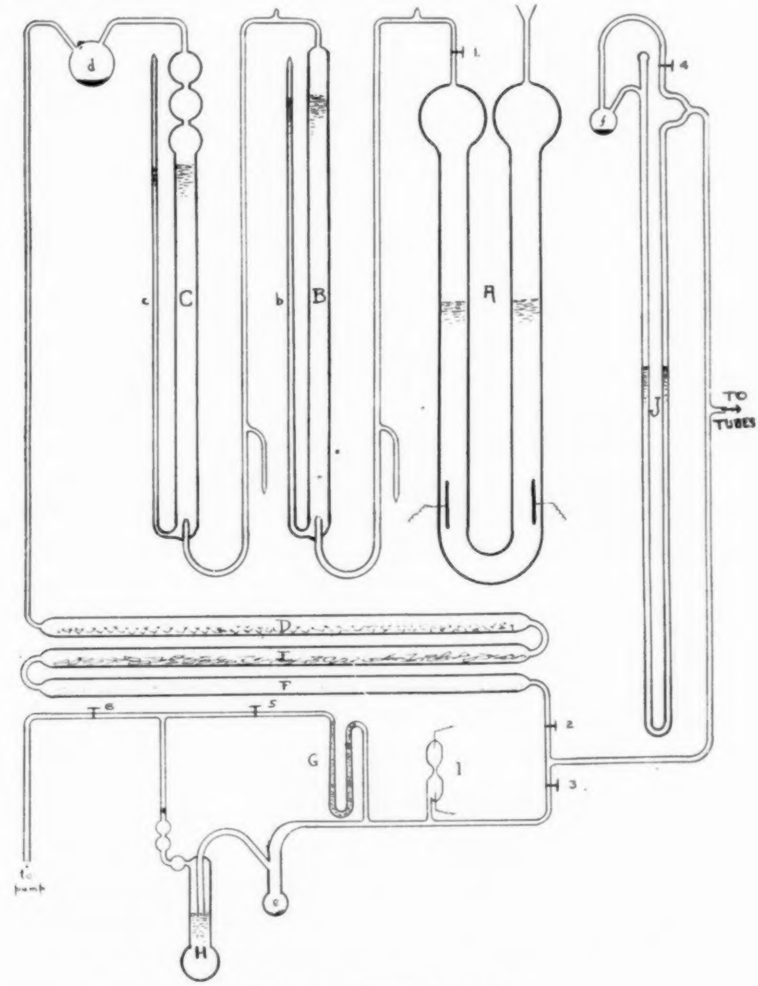


Fig. 1.—Hydrogen Generator

45 cc water; 360 gms potassium hydrate (prepared without use of alcohol) in 240 cc water. These two solutions are made separately, then thoroughly mixed, and quickly transferred to the tubes

prepared for them without any of the solution which entered the tubes having come in contact with the air. A very brief exposure to air will oxidize the solution and render it almost worthless. To avoid the presence of air in the apparatus at the start it was all completely exhausted before any of the liquids were admitted to it. The side tubes *b* and *c* make it possible to replenish the solutions when exhausted without contamination of the contained gas and without using stopcocks. The hydrogen, when thus freed from oxygen was collected in the tubes *D*, *E*, and *F*, which contained, respectively, calcium chloride, potassium hydrate sticks, and phosphorous pentoxide, and which serve as driers and as a storage chamber.

The stopcock 2 admits the gas to the experimental tubes which are in connection with a self-exhausting sulphuric acid manometer, *J*. When cock 4 in the manometer is open both sides can be completely exhausted, after which the cock is closed. On readmitting gas the difference in level assumed by the two arms indicates the pressure. The trap *f* serves to protect the cock from acid accidentally thrown over by the use of too high a pressure. Exhausting the apparatus takes place through cock 3 by means of a Gaede pump. To prevent diffusion of mercury vapor back from the pump into the discharge tubes two mercury traps, *G* and *H*, were provided. For all pressures down to .03–.04 cm cock 5 was left closed and the trap *H* used. This consisted simply of a tube just dipping beneath the surface of sulphuric acid. Exhaustion through this could take place down to a pressure approximately represented by the amount of submersion of the tube. For pressures below this amount cock 5 was opened and exhaustion took place through tube *G*, which was closely packed with gold leaf. At the low pressures for which *G* was needed the free path of the mercury molecules was large enough to prevent their diffusion through the spaces between the gold without hitting it. Thus all are caught and held by the latter. Obviously at higher pressures this might not be the case. These two traps were completely successful in preventing the diffusion of mercury back into the apparatus. During a year's intermittent use the test discharge tube at *I* never showed any trace of the mercury spectrum.

Fig. 2 shows a general plan of the rest of the apparatus. The light from the discharge tube, *D*, was compared with that from a Nernst glower, *N*, by a modified form of a Brace spectrophotometer, *B*. The selective absorption of the silver strip through the middle of the photometer prism was found to be negligible for such measures as these experiments involved. Constant slit-widths were used in both collimators, but the beam coming from the Nernst was plane polarized by means of a Nicol at *P*. After having traversed

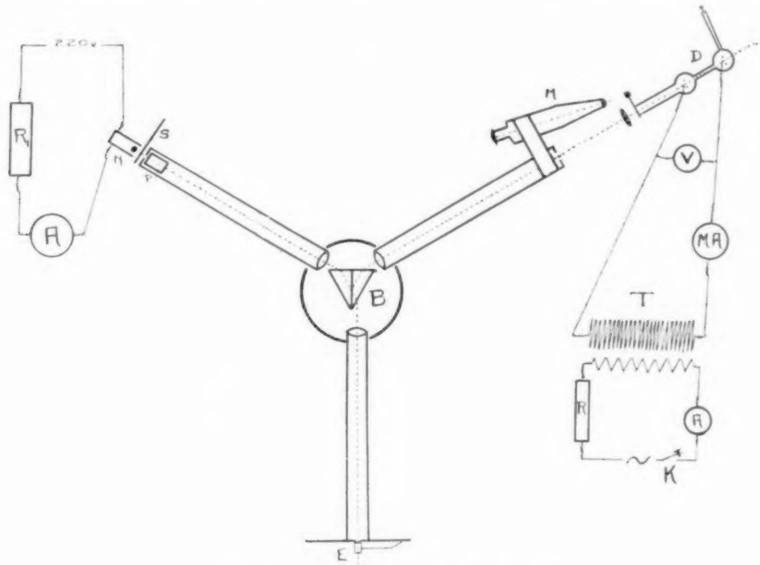


Fig. 2.—General Plan of Apparatus

identical paths through the prism (except for the reflection) the two beams unite in the telescope and the two spectra formed superimpose when viewed with an eyepiece at *E*. An eye-slit at this point cuts out all light except that of the spectral line or group being measured, and the background behind it. On removing the eyepiece and looking through the eye-slit with the eye approximately in the principal focus of the telescope lens the surface of the prism appears illuminated uniformly, except for the silver strip, which is of different intensity from the rest. Now by means of an analyzing Nicol in front of the eye the intensity over the rest of

the surface can be made to match exactly that of the silver strip, when, by careful adjustments, the latter disappears. The speed and accuracy of the settings obtainable with this form of spectrophotometer are too well known to require mention.

The Nernst comparison lamp, *N*, was a seasoned 90 v., 0.5 amp. glower held by means of a series resistance, R_1 at a constant current. A heavy metal shutter, *S*, screened the collimator slit from the intense radiation except while settings were actually being made. The discharge tubes had the form shown in section in Fig. 3. The

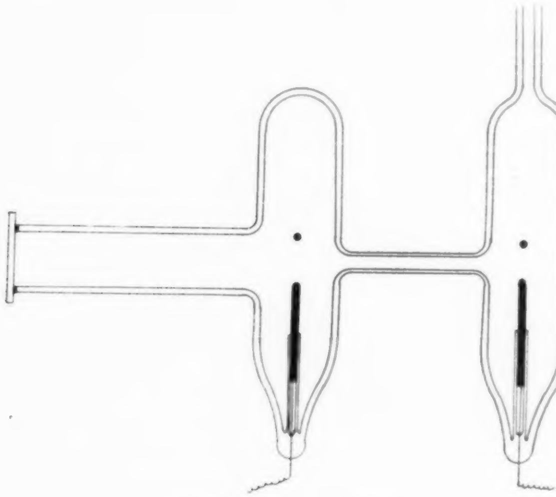


Fig. 3.—Discharge Tube

top part of the electrodes was bent into the form of a ring so that the light from the capillary could be viewed "end-on" through the side arm, which was closed with a quartz or glass window. The tubes were excited by means of the secondary circuit of a 5 kw. transformer (see Fig. 2). This circuit also contained as indicated a Weston milliammeter and a Brown electrostatic voltmeter. The primary of the transformer was fed through a series resistance, *R*, with alternating current from a 5 H.P. twin motor-generator set which had separately excited generator fields and could be held very constant. In fact, the current through the tubes never fluctuated by amounts readable on the milliammeter. An open

circuit program of observations was adopted, the switch K being open and no current flowing through the tubes except at the moments settings were being made. Thus there was no heating of the tubes due to the discharge.

Several of the tubes were held simultaneously in a double-walled container which permitted the part containing the discharge to be entirely surrounded with liquid air when desired. The side arm of the tube, projecting through the walls of the container, gave an uninterrupted view into the interior of the tube at all times. On the window was attached by means of hard wax a fine needle point which was observed in a micrometer microscope, M, clamped to one of the collimators. Any shift between collimator and tube was, therefore, easily detected and carefully corrected by means of adjusting screws. Moreover, the various tubes in the container could thus be successively brought into alignment, moved out, and brought back again accurately to their original positions. Reliable measures were found impossible without this arrangement, since the changes in temperature to which the container was subjected would warp it slightly, thus bringing into view light from a slightly different portion of the capillary and causing thereby variations of selective intensity considerably larger than those due to the causes under consideration.

Method of observation.—The plan of observation finally adopted as the most reliable was to make at a given pressure two successive comparisons of the radiation of the discharge tube at room temperature with that of the Nernst glower. Then after cooling the tube two successive comparisons were again made with the glower, the gas density in the tube being kept the same and the electrical conditions remaining constant throughout. Fresh, spectroscopically pure gas was readmitted for every set of observations. The gas density was controlled in two ways: (1) Gas was admitted to the tube at the desired pressure and at room temperature, and the tube then sealed off from the rest of the apparatus. Observations were then made first at room temperature then at that of liquid air. Owing to the possibility that occlusion at the low temperature might change the gas density, only few observations were made using this method. (2) In most of the observations gas was

admitted to the tubes to the desired pressure and observations with the tube at room temperature taken. The tube was then cooled while remaining in connection with pump and gas supply and the pressure now so adjusted that the gas density was just the same as before, i.e., the pressure was made proportional to the absolute temperature. Observations at the low temperature were then made. No difference was observed in measurements resulting from these two different methods of control, except at very low pressures.

The current through the tubes being kept constant and the mass of gas between the electrodes being always the same, the potential difference between the electrodes would not be expected to change. Earhart[1] has shown, however, that the behavior of hydrogen is irregular in this respect, a fact which has been also observed in these experiments. For pressures below 6–7 mm the potential difference across the electrodes rises somewhat when the tube is cooled. At pressures above this value it falls somewhat on cooling. Test experiments have shown, however, that these variations are wholly incapable of producing in the radiation from the tubes such large changes as have been observed.

To sum up then: Under electrical conditions which have been controlled as closely as the nature of the case permits, indeed to within a few per cent, measurement has been made of the effect of a decrease in the temperature of the discharge tube from about 300° absolute to about 100° absolute, upon the photometric intensities of those parts of the hydrogen spectrum above mentioned, i.e., the series lines, α, β, γ ($\lambda = .656, .486, .434\mu$), the secondary groups, a, and b ($\lambda = .601, .493\mu$ approx.), and the continuous ground immediately adjacent to α, β, and γ, which is very near also to a, and b. The readings actually taken are successively on the intensities of (1) $a+\alpha_g$; (2) α_g; (3) $a+\alpha_g$; (4) $b+\beta_g$; (5) β_g; (6) $\beta+\beta_g$; (7) γ_g; (8) $\gamma+\gamma_g$. The subtraction of (2) from (1) and (3), of (5) from (4) and (6), and of (7) from (8) give the true values of α, β, γ, a, and b corrected for background. (2) has always been observed equal to zero and therefore has been omitted from the tables.

[1] "The Effect of Temperature on Electrical Discharge in Gases," *Physical Review*, **31**, 652, 1910.

Results.—A summary of the results obtained is given below. Three different tubes were used which differed in general dimensions and especially in the bore of the capillary. Under a total current of 6 m.a. the different sized capillaries resulted in the following current-densities for the different tubes:

Tube 1.	2.3 amp/cm²
Tube 0.	0.2 "
Tube 2.	0.04 "

Five different pressures were used, viz., 0.08, .13, .31, .70, 1.37 cm (*Hg*) respectively. The figures tabulated are the ratio

Intensity at T = 100°: Intensity at T = 300°.

Two independent measures of the numerator and two of the denominator were made. The four possible combinations of these which give the ratio are shown, as illustrating the experimental error, and the average taken. The total number of settings involved in the data of each table is given with it.

TABLE II

A

PRESSURE, 0.08 CM — TUBE 2

Current-Density, 0.04 amp/cm² — 140 Settings

α	β	γ	a	b	β_g	γ_g
.51	.55	.40	1.55	1.64	1.17	1.35
.49	.56	.38	1.60	1.47	1.22	1.24
.55	.50	.52	1.41	1.70	1.33	1.27
.53	.51	.49	1.45	1.52	1.38	1.17
Av. .52	.53	.45	1.50	1.58	1.27	1.26

B

PRESSURE, 0.08 CM — TUBE 1

Current-Density, 2.3 amp/cm² — 105 Settings

α	β	γ	a	b	β_g	γ_g
.58	.31	.39	1.22	1.24	1.13	1.07
...	...	...	1.33	1.28	1.13	1.04
Av. .58	.31	.39	1.27	1.26	1.13	1.05

TABLE III

A

PRESSURE, 0.18 CM — TUBE 2

Current-Density, 0.04 amp/cm² — 210 Settings

α	β	γ	a	b	β_g	γ_g
.64	.52	.64	1.36	1.21	1.30	1.28
.61	.62	.57	1.41	1.31	1.28	1.28
.53	.54	.53	1.36	1.08	1.19	1.23
.51	.63	.57	1.41	1.17	1.17	1.23
(.48	.40	.87	1.40	1.02	1.28	1.19)
Av. .56	.56	.58	1.38	1.18	1.24	1.24

B

PRESSURE, 0.18 CM — TUBE 1

Current-Density, 2.3 amp/cm² — 140 Settings

α	β	γ	a	b	β_g	γ_g
.28	.28	.33	1.18	1.54	1.57	1.38
.28	.33	.34	1.30	1.47	1.57	1.77
.32	.30	.40	1.09	1.63	....	....
.32	.26	.41	1.20	1.56	....	....
Av. .30	.32	.37	1.19	1.55	1.57	1.57

TABLE IV

A

PRESSURE, 0.32 CM — TUBE 2

Current-Density, 0.04 amp/cm² — 280 Settings

α	β	γ	a	b	β_g	γ_g
.51	.56	...	2.00	1.85	1.59	1.40
.52	.56	...	2.00	2.16	1.40	1.47
.42	.50	...	1.57	1.78	1.68	1.40
.43	.50	...	1.58	2.08	1.48	1.47
Av. .47	.53	...	1.78	1.97	1.54	1.44

B

PRESSURE, 0.32 CM — TUBE 1

Current-Density, 2.3 amp/cm² — 140 Settings

α	β	γ	a	b	β_g	γ_g
.37	.52	.45	1.12	1.30	1.00	0.92
.36	.49	.37	1.12	1.30	1.00	0.92
.38	.53	.44	1.00	1.38	0.95	1.03
.37	.50	.35	1.00	1.38	0.95	1.03
Av. .37	.51	.40	1.06	1.34	0.97	0.97

C

PRESSURE, 0.32 CM — TUBE 0

Current-Density, 0.2 amp/cm² — 140 Settings

α	β	γ	a	b	β_g	γ_g
.64	.74	...	1.51	1.55	1.49	1.11
.64	.79	...	1.41	1.52	1.29	1.21
.67	.59	...	1.45	1.52	1.43	1.07
.67	.63	...	1.33	1.30	1.27	1.17
Av. .66	.69	...	1.42	1.42	1.36	1.14

TABLE V

A

PRESSURE, 0.7 CM — TUBE 2

Current-Density, 0.04 amp/cm² — 210 Settings

α	β	γ	a	b	β_g	γ_g
.62	.47	...	2.47	1.78	1.33	1.42
.62	...	...	2.56	....	1.25	1.37
.67	.83	...	2.44	1.99	1.34	1.42
.66	...	...	2.52	....	1.26	1.37
.60	.87	...	2.37	1.73	1.32	1.42
.59	...	...	2.46	....	1.24	1.37
Av. .63	.71	...	2.47	1.83	1.29	1.40

B

PRESSURE, 0.7 CM — TUBE 1

Current-Density, 2.3 amp/cm² — 70 Settings

α	β	γ	a	b	β_g	γ_g
.45	.38	.60	1.93	1.84	1.94	2.10
.31	.43	.55	1.86	1.95	1.56	1.96
Av. .38	.40	.58	1.90	1.90	1.75	2.03

TABLE VI

A

PRESSURE, 1.37 CM — TUBE 2

Current-Density, 0.04 amp/cm² — 140 Settings

α	β	γ	a	b	β_g	γ_g
.47	.23	...	2.38	2.44	1.25	1.02
.50	.34	...	2.24	....	1.00	0.98
.31	.45	...	1.94	1.69	1.20	0.97
.33	.66	...	1.82	....	0.96	0.94
Av. .40	.40	...	2.10	2.06	1.10	0.97

TABLE VII

SUMMARY

Current-Density, 0.04 amp/cm² Tube 2

Press.	α	β	γ	a	b	β_g	γ_g
.08 cm	.6	.5	.4	1.5	1.6	1.3	1.3
.18 "	.6	.6	.6	1.4	1.2	1.2	1.2
.31 "	.5	.5	..	1.8	2.0	1.5	1.4
.70 "	.6	.7	..	2.5	1.8	1.3	1.4
1.37 "	.4	.4	..	2.1	2.1	1.1	1.0

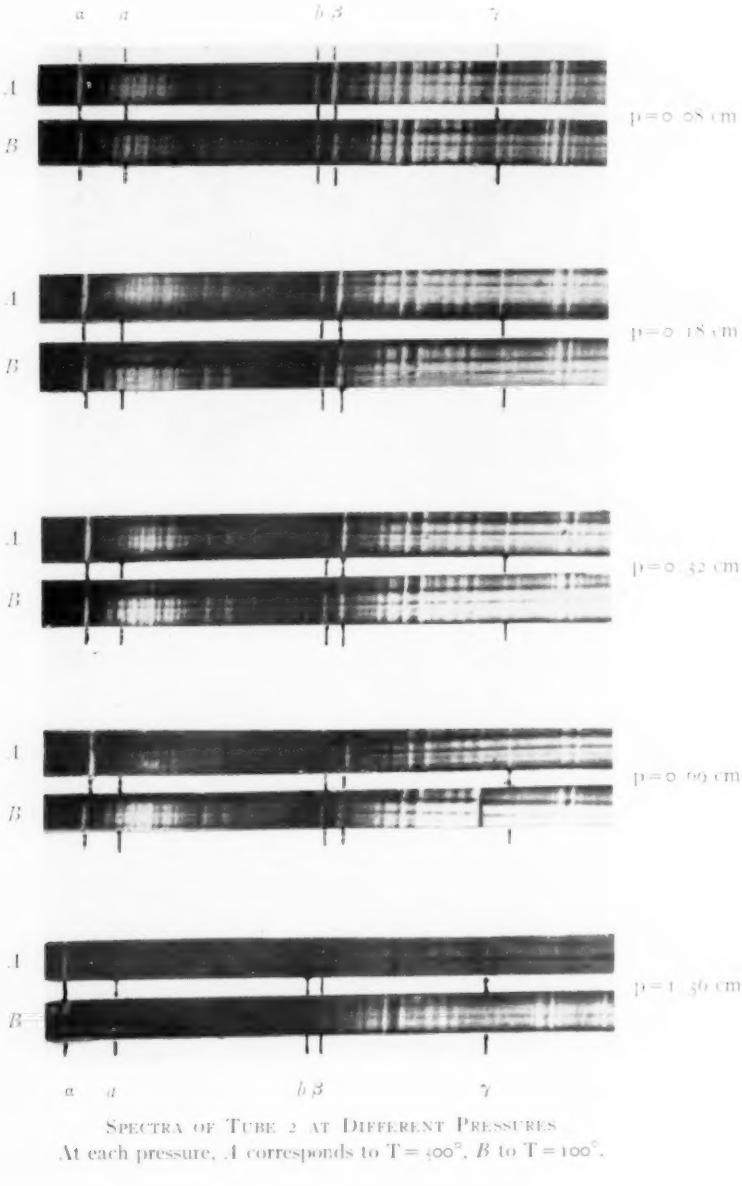
Current-Density, 2.3 amp/cm² Tube 1

Press.	α	β	γ	a	b	β_g	γ_g
.08 cm	.6	.3	.4	1.3	1.3	1.1	1.1
.18 "	.3	.3	.4	1.2	1.5	1.6	1.6
.31 "	.4	.5	.4	1.1	1.3	1.0	1.0
.70 "	.4	.4	.5	1.9	1.9	1.8	2.0

It is seen from these tables, summed up in Table VII, that the results, for all pressures and the two widely different values of current-density used, are in quite good agreement. In all cases at the lower temperature, the series spectrum weakens to about 0.5 its normal value, and the compound line spectrum strengthens to 1.5 or 2.0 times its normal value, while the continuous background increases somewhat, but less than the compound spectrum. Inasmuch as the background consists of true continuous spectrum plus some faint compound spectrum, it is quite possible that the true background changes little if at all, the faint compound spectrum mixed with it being responsible for the slight rise shown.

The radiant energy, then, as the discharge tube is cooled, tends to leave the series lines and to go into the compound line spectrum. This is in accord with the general behavior of series and many-lined spectra. The former are characteristic of substances which radiate at temperatures considerably above their melting or boiling points or under the most powerful electrical conditions; the latter are characteristic of substances such as iron, titanium, etc., whose condition when radiating approaches the solid or liquid state. Continuous spectra are characteristic of glowing solids and liquids. Hydrogen exhibits simultaneously these three kinds of spectra.

PLATE VIII



SPECTRA OF TUBE 2 AT DIFFERENT PRESSURES
At each pressure, *A* corresponds to T = 300°, *B* to T = 100°.

When its temperature falls and approaches those at which liquefaction can occur its predominating spectra become those characteristic of substances near the liquid or solid conditions, i.e., the compound line spectrum and the continuous spectrum, to a less degree. The photograph, Plate VIII, of the spectra of tube 2, shows clearly this transfer of energy from the series spectrum to the many-lined spectrum with a decrease in the temperature. Five pressures, approximating those at which the visual observations were taken are shown. The top spectrum, *A*, in each case is taken with the discharge tube at room temperature, the bottom one, *B*, with the tube immersed in liquid air. The positions of the series lines, α, β, γ, are marked as well as those of the groups *a* and *b* of the compound spectrum. The relative weakening of the series lines and strengthening of the many-lined spectrum at the low temperature, *B*, is very clear. Also as the pressure rises the large increase in the amount of continuous spectrum present is illustrated.

As to the behavior of the series lines with reference to each other, it is seen to be the same for all, to within the errors of measurement. No selective changes in their intensities are observed. In tube 2, which is the more reliable, the current-density was not high enough to bring γ into visibility for measurement at pressures higher than .3 cm. In tube 1, however, γ is observed throughout, and in all cases where there is enough data to rely upon the intensities of α, β, γ, respectively, all changed in the same way and to the same degree. Preliminary experiments which have not been made use of in this work pointed to the same result.

It is concluded, therefore, that as regards the effect of temperature on the spectrum of hydrogen in a vacuum tube, excited by alternating current, (1) a variation in the temperature of the discharge tube of from 300° to 100° Abs. largely affects the relative amounts of energy in the series and in the compound line spectrum, the latter being much enhanced at the lower temperature, (2) this variation of temperature does *not* affect the relative intensities of the series lines at least to within the limits of error of the experiment, which have been guarded by a painstaking control of electrical conditions and careful photometric measurements.

The author wishes to acknowledge his indebtedness to the

members of the staff of the Ryerson Physical Laboratory, and especially to Professor A. A. Michelson, for the interest taken in this investigation and for the ample facilities that have been placed at his disposal. Part I*a* of the work, herein described, was originally suggested by Dr. Henry Gale. To the firm of William Gaertner & Co. are due thanks for the loan of the excellent Brace spectro-photometer which was the basis for the modified form of instrument used.

RYERSON PHYSICAL LABORATORY
August 1911

SPECTRA AND COLORS OF RED STARS

By J. A. PARKHURST

The writer has been engaged since 1906 in determining the relation between the spectra and color-indices of stars, and has found a good agreement between the two, at least as far as Type III or Harvard Class M; so good that the Harvard spectral classes and color-indices, when platted, fall nearly in a straight line. The slope of this line is in fair agreement with that found by E. S. King[1] from brighter stars.

Exceptions to this rule are therefore of special interest and have been investigated by the writer at various times, usually with negative results. For example, some of the most striking exceptions in the list published by Franks[2] were found to be quite normal. A list of "Fourth-Type Stars not Red" given in a letter from Professor E. C. Pickering, dated June 5, 1911, therefore demanded attention. In order to get a better idea of the spectra of a variety of stars of Type IV, the list was extended from *Harvard Circular*, No. 145, "Stars having Spectra of Type VI, Class R," also from a list of red stars whose spectra had been photographed with the 40-inch Yerkes refractor, and the results published by Hale, Ellerman, and the writer in 1903.[3] This extension of the list was made possible by the efficient assistance rendered by Professor A. H. Joy of the Syrian Protestant College of Beirut, Volunteer Research Assistant for the summer of 1911; and of Mr. C. H. Gingrich, Fellow in Astronomy at this observatory.

Ellerman had photographed the spectra of the brighter stars of Type IV in two parts; the blue region on Cramer Crown and Seed 27 plates, and the yellow region on Cramer Isochromatic plates at a different setting of the Brashear spectrograph, using a camera of 271 mm focal length, and three 60° prisms. For reproduction these spectra were enlarged, and at the same time broad-

[1] *Harvard Annals*, **59**, 180. [2] *Monthly Notices*, **70**, 191, 1909.

[3] "Spectra of Stars of Secchi's Fourth Type," *Publications of the Yerkes Observatory*, **2**.

ened by a pendulum motion of the plate, so that they showed a great number of fine lines, very much like spectra of Type II. These spectra are therefore not as well adapted to account for the color-indices of the stars as if the blue and yellow regions had been taken on the same plate. Nevertheless, being the only large-scale spectra of extremely red stars so far published, they are very useful in themselves, and a comparison with the objective-prism spectra is given later in Plate IX, Fig. 1, to aid in co-ordinating the two sorts of spectra. Some specimens of the spectra taken with the 40-inch telescope therefore follow.

Fig. 1 includes the blue region of Types II, III, and IV, the stars, reading from the top downward, being *280 Schjellerup*, Type IV; Sun, Type II; *μ Geminorum*, Type III, and *74 Schjellerup*, an advanced specimen of Type IV. A close correspondence between the finer metallic lines in all four spectra is evident, but the prominent characteristic of the spectra of Type IV is the broad dark band extending from about λ 4650 to λ 4750, with a bright band of nearly equal width on the less refrangible side and a less intense but broader bright region on the more refrangible side.

The yellow region of the same spectra is shown in Fig. 2. Here the correspondence in the metallic lines is somewhat masked by the carbon flutings in the spectra of Type IV. The most prominent characteristic of the red stars in this region is the bright band having its more refrangible edge sharply defined at about λ 5640.

An inspection of these two figures will show that the color-index of stars of Type IV will depend mainly on the strength of the bright band in the yellow as compared with the bright regions bordering the dark band in the blue.

In Fig. 3 we have a comparison of Types II and IV extending farther into the violet, to about λ 4200, and in Fig. 4 a still greater extension to a little beyond λ 3900. These show a falling off in intensity of the Type IV spectra at λ 4300 (the solar G group) and another at λ 4227. Fig. 4 is remarkable in that it shows the extension of the spectrum of the red star *19 Piscium* beyond the Fraunhofer H and K lines. This plate was taken by Ellerman with the Brashear spectrograph on the 24-inch reflector, requiring an exposure of 24 hours and 40 minutes on four nights.

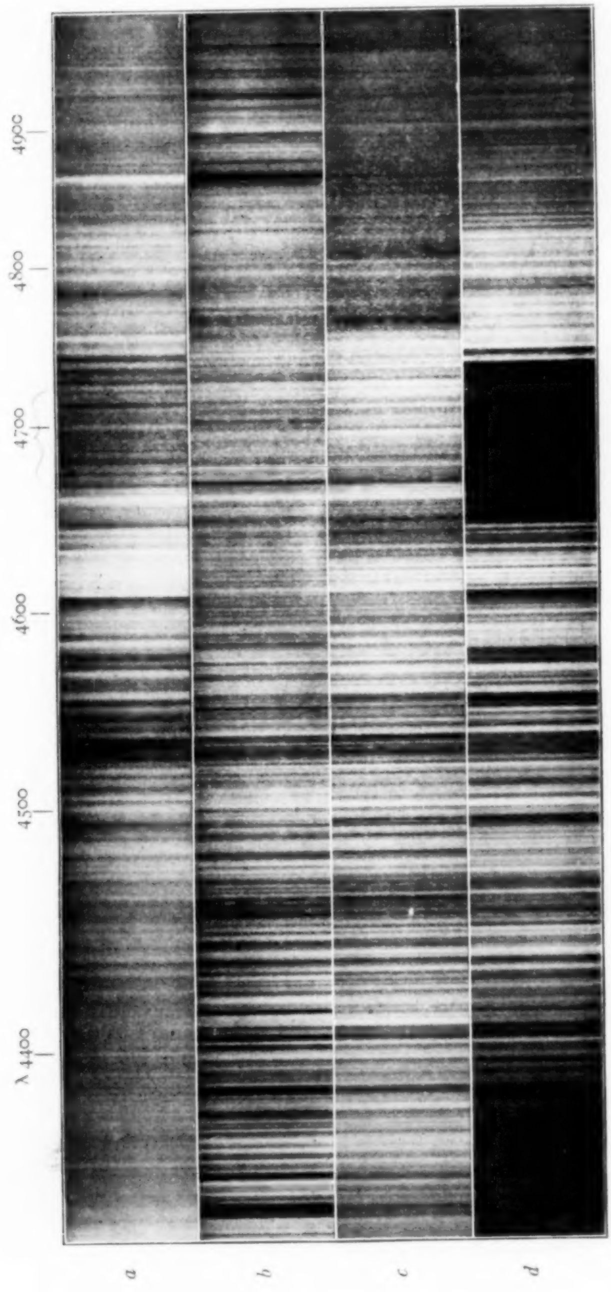


FIG. 1.—Blue Region of Stars of Types II, III, and IV

(*a*) *280 Schj.* (*b*) Sun. (*c*) *μ Geminorum.* (*d*) *74 Schj.*

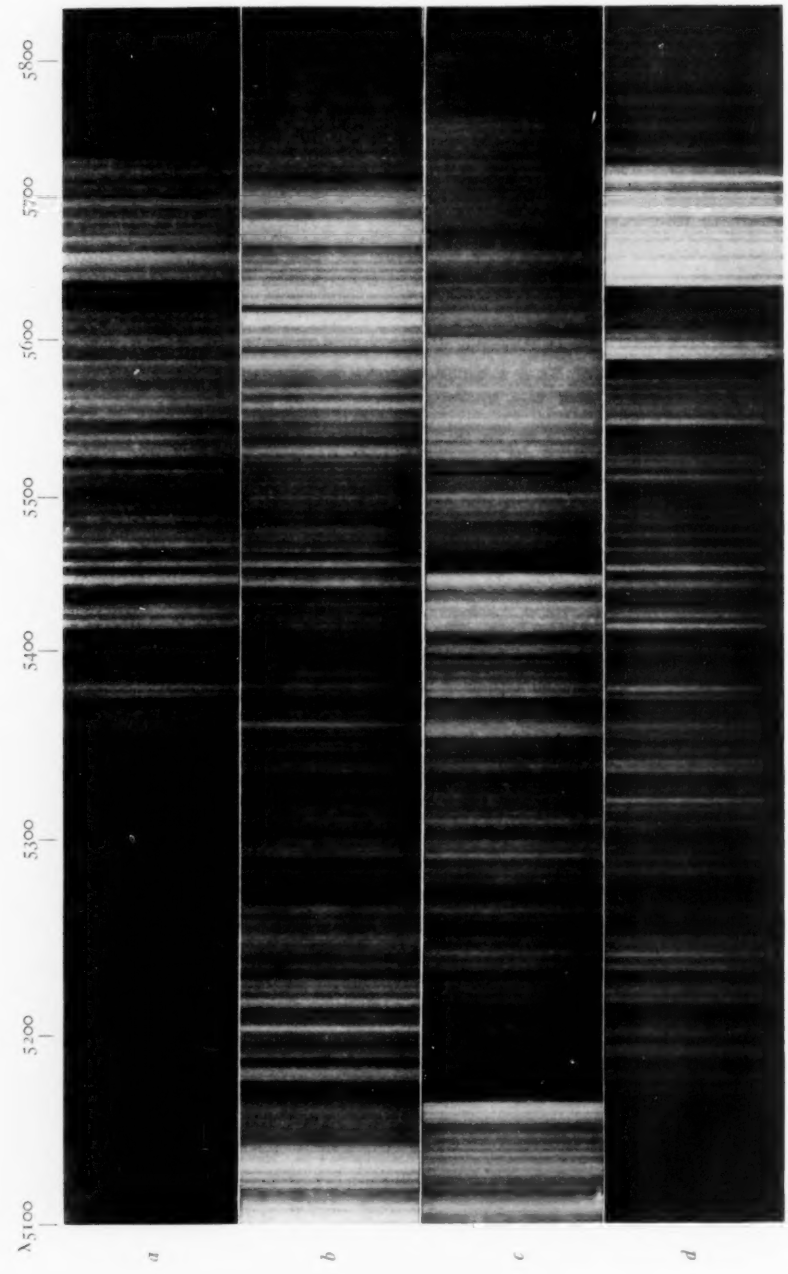


FIG. 2.—Yellow and Green Regions of Stars of Types II, III, and IV
(*a*) 280 *Schj.* (*b*) Sun. (*c*) μ *Geminorum.* (*d*) 74 *Schj.*

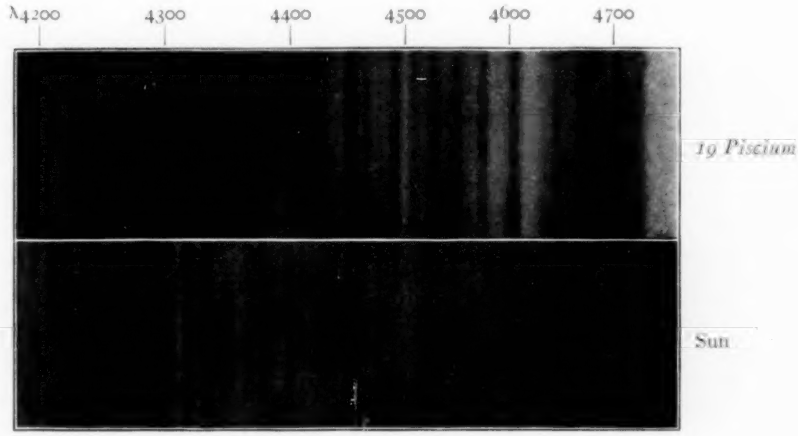


FIG. 3.—Blue Region of Types II and IV

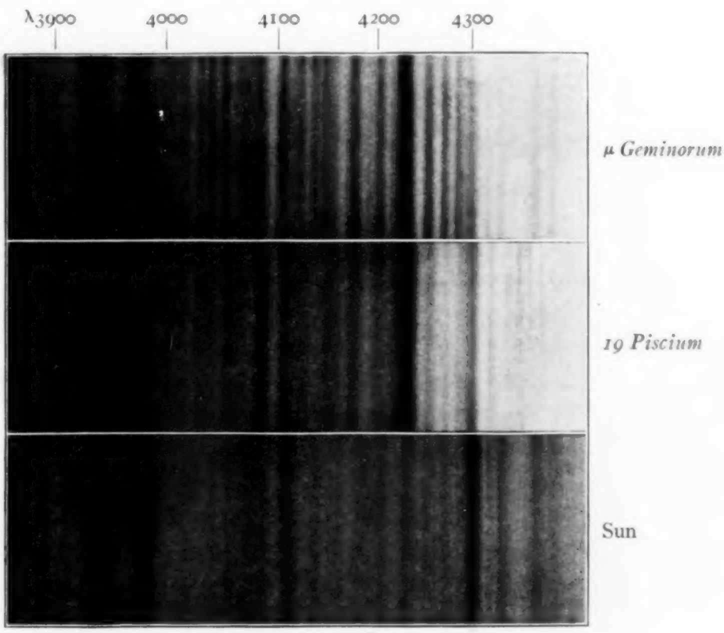


FIG. 4.—Violet Region of Types II, III, and IV

Fig. 5 shows both the blue and yellow regions of a series of stars of Type IV. In the blue region the regular progression in intensity of the dark λ 4700 band suggests an evolutionary series in the stars. This progression is not as evident in the yellow region, though it probably exists there also. This figure is a composite of Plates VIII and IX in the paper above quoted in Vol. II of the Yerkes Observatory Publications. The separation between the blue and yellow regions in the figure corresponds quite closely with the green region in the spectrum to which the plates used were not sensitive, and which therefore does not appear on the photographs. The star

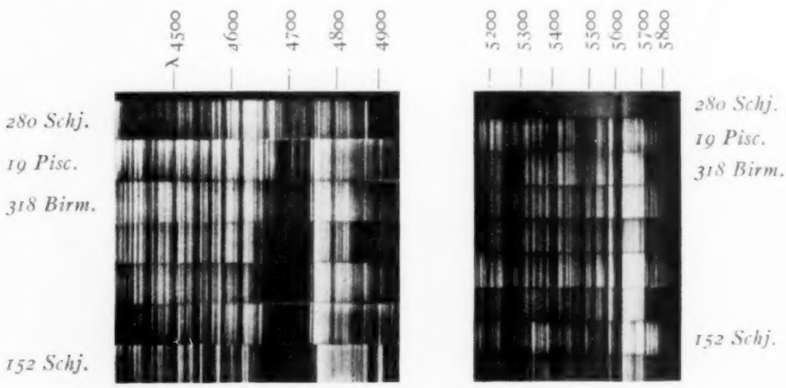


Fig. 5.—Spectra of Fourth-Type Stars Taken with Slit Spectrograph

names shown on the margins of the figure are those for which values of the color-index are given in the table following.

The spectra of most of these red stars were photographed with the Zeiss camera of 145 mm aperture and 814 mm focal length, provided with a doublet lens and 15° objective-prism of ultra-violet glass. These spectra were broadened by the diurnal motion to a width of about 0.6 mm, sufficient to show the lines. To give an idea of the relative appearance of the 40-inch and the objective-prism plates, the blue region of the spectrum of *19 Piscium* has been enlarged to the same scale from plates of each kind, and the two shown in Plate IX, Fig. 1. The objective-prism plates have a greater extension in each direction, but show only the features which are most prominent on the 40-inch plates. The broad

PLATE IX

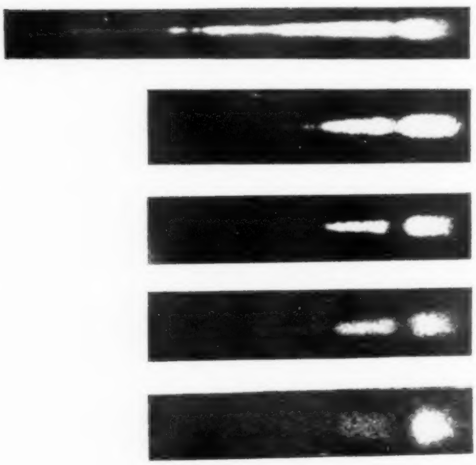
λ 4300 4700

Fig. 1 *19 Piscium*

Objective-Prism

Slit-Spectrograph

Fig. 2



−10° 5057

19 Piscium

240a Schj.

318 Birm.

152 Schj.

Spectra of Fourth-Type Stars

absorption band at λ 4700 is well shown on both, and the objective-prism plate shows the fall in intensity at λ 4300 and the extension of the spectrum to λ 4227. The similarity between the spectra of Types II and IV is thus evident in these bolder features as well as in the agreement of the fine metallic lines.

Fig. 2 of Plate IX shows a series of objective-prism spectra of red stars of Type IV, beginning with the short spectrum of *152 Schjellerup* at the bottom, and ending with the long spectrum of *B.D.* $-10°5057$ at the top. The two principal points to be noticed in this series are: first, the difference in the relative intensity of the light-action on each side of the dark λ 4700 band; and second, the difference in the length of the spectra. As the conditions of plate, exposure, and development were similar, these represent real differences in the stars. Going up from *152 Schjellerup*, the light on the red side of λ 4700 steadily diminishes in intensity as compared with the light on the violet side of the band. The upper spectrum, *B.D.* $-10°5057$, shows the Fraunhofer H and K lines and extends some distance beyond; but as Fig. 4 showed the spectrum of *19 Piscium* to H and K, it is probably a mere question of exposure to extend the other spectra to this region, except perhaps that of *152 Schjellerup*.

The upper spectrum, that of *B.D.* $-10°5057$, requires further mention, since the negative, even more than the engraving, suggests a composite spectrum. Mrs. Fleming seemed to have the same idea, for she wrote:[1] "The lines in the spectrum are not those due to hydrogen. In some photographs they are broad bands, while in others the lines appear to be double." Professor Barnard kindly examined the star with the 40-inch telescope, but could detect no duplicity. The spectrum resembles the solar type, with the addition of the dark λ 4700 band. The color-index, given in the following table, is 1.09, but little greater than that of an ordinary solar type star.

In the accompanying table of spectra and color-indices, the stars are arranged in order of the values of the color-index. The first column gives the name of the star, the second the *B.D.* designation, the third and fourth the place for 1900, the fifth the *B.D.* magnitude,

[1] *Astronomische Nachrichten*, **128**, 122, 1891.

the sixth the color and magnitude from the Potsdam Durchmusterung. The spectral classification given in the seventh column is usually from the Harvard lists, given as N, Na, or R. For the star *3 Schjellerup* Duner's classification, IV is used. The color in the eighth column, when expressed by one figure means Wendell's estimate made visually with the 15-inch equatorial, and communicated in Professor Pickering's letter. When a decimal place is given, the estimate is Duner's, taken from Krüger's *Catalog der farbigen Sterne*. The ninth column gives the color-index in magnitudes, as obtained by methods which will be presently explained. The tenth column gives the number of plates, R standing for the 24-inch reflector and C for the Zeiss camera.

SPECTRA AND COLOR-INDICES OF RED STARS

1 Star	2 *B.D.*	3 1900 R.A.	4 Dec.	5 *B.D.* Mag.	6 P.D. Col. Mag.	7 Spectrum	8 Color	9 Color-Index	10 Plates R.	 C.
		h m	° ′					M		
	$-10°$ 5057	19 13	−10 53	7.0	....	R pec	...	1.09	4	3
	+85 332	19 44	+85 9	9.2	....	......	...	1.56	4	
	+53 66	0 19	+53 46	9.3	....	R	...	1.66	4	
	+20 5071	22 0	+20 34	8.7	....	R	...	1.82	4	
	+38 2389	12 55	+38 22	8.6	....	N	1	1.94	6	
	−16 5272	19 13	−16 5	6.8	....	Na	...	2.18	4	
	+ 6 3898	18 37	+ 6 43	9.0	....	......	...	2.37	4	
152 Schj...	+46 1817	12 40	+45 59	5.5	GR-5.24	Na	8.0	2.50	8	4
74 Schj...	+14 1283	6 20	+14 47	6.5	GR 6.26	N	...	2.54	2	3
	+76 734	19 25	+76 22	6.5	GR 6.24	......	...	2.63		5
249a Schj..	+34 4500	21 38	+35 3	6.2	GR 6.13	Na	4	2.74	6	4
19 Piscium	+ 2 4709	23 41	+ 2 56	6.2	GR 5.12	Na	...	2.80	4	3
280 Schj...	+59 2810	23 56	+59 48	7.8	....	Na	6	2.88	5	
318 Birm..	+68 617	10 38	+67 56	6.1	RG 6.07	N	8.3	3.18	4	4
	+36 3243	18 39	+36 52	7.6	RG-7.62	Na	5	3.26	5	
3 Schj...	+43 53	0 15	+44 9	8.2	....	IV	9.3	4.56	4	
U Cygni...	+47 3077	20 16	+47 35	var	....	pec	...	5.60	4	

The "color-index," the difference between the photographic and visual magnitude, was determined as follows: The photographic magnitudes were found from Seed 27 and 30 plates, those with the reflector being taken in focus, those with the camera being extrafocal. The "visual" magnitudes were found with Cramer Trichromatic plates taken in focus behind a Wallace "visual luminosity" filter, both with the reflector and the camera. The

extra-focal images were measured with the Hartmann "Mikrophotometer" and reduced with the writer's absolute scale. The diameters of the focal-images were measured under the microscope and reduced by the formula

$$\text{Mag.} = a - b\sqrt{D}.$$

The following conclusions can be drawn from the Table:

1. The three stars classed as having spectrum R in the Harvard lists have the smallest values of the color-index.

2. The nine stars classed N and Na have colors ranging from 1.94 to 3.26. Among these, Wendell's estimates of color range from 1 to 6, nearly in the order of the color-index.

3. Duner's color estimates, 8.0, 8.3, and 9.3, correspond to color-indices 2.50, 3.18, and 4.56; a decided difference in the range.

4. The star *B.D.* $-10°5057$ stands in a class by itself.

5. There seems to be no sharp dividing line between Harvard Classes R and N.

6. Except the first two, all the stars in the list are as red or redder than *α Tauri* (for which E. S. King gives the color-index 1.64), so that the expression "Fourth-Type Stars not Red" seems inappropriate.

Yerkes Observatory
February 1912

THE PARALLAX OF *NOVA LACERTAE* 1910

By FREDERICK SLOCUM

Nova Lacertae was discovered by Espin December 30, 1910. The telegram announcing the discovery was received at Williams Bay on the afternoon of December 31, and the first plate for the investigation of its parallax was obtained with the 40-inch telescope soon after 6 o'clock that same evening.

The longitude of the star was 91° greater than that of the sun on that date, giving practically the maximum negative value for its parallax factor, −0.98.

Ten plates were secured as shown by Table I. The first two were taken when the star was east of the sun, the next four when it

TABLE I

PLATES OF *Nova Lacertae*

TAKEN WITH THE 40-INCH REFRACTOR OF THE YERKES OBSERVATORY

No.	Date	Hour Angle	Observers	Quality of Images	Remarks
359	1910 Dec. 31	$+2^{h}8$	Sl, Sl	Fair	Telescope east
360	1911 Jan. 7	+2.8	Sl, Sl, Sl	Good	Telescope east
432	June 17	−2.0	Su, Sl	Good	
438	July 1	− .6	Su	Fair	
446	July 8	− .5	Su, Sl	Good	
498	Sept. 16	0	Sl, Su	Good	
531	Oct. 14	− .2	Sl, Su	Good	
536	Oct. 28	− .5	Su, Sl	Good	
548	Nov. 21	− .1	Sl, Sl	Good	
563	Dec. 1	− .2	Sl, Sl	Good	

was west of the sun, and the last four again to the east. The third column gives the hour angle of the star at the time the plate was exposed, plus indicating west of the meridian, and minus, east. So far as possible all observations for parallax are made near the meridian with the telescope west of the pier. In the case of the first two plates of this series, taken just after the *Nova* was discovered, the star had already passed the meridian by the time the sky was dark enough to allow a plate to be exposed, and it was

necessary to reverse the telescope and make the exposure with a relatively large hour angle, 2.8 hours.

In general two exposures were made on each plate, the guiding being done by the observers indicated in column 4: Sl=Slocum, and Su=Sullivan. On plate No. 360 there were three exposures and on No. 438 only one.

Six comparison stars were selected, as symmetrically situated as possible, and as near as possible to the mean brightness of the *Nova*. Table II gives additional data pertaining to the comparison

TABLE II

COMPARISON STARS

No.	Diameter	Approx. Magnitude	X (Longitude)	Y (Latitude)	Dependence
1	0.32 mm	8.8	−261.6	− 77.2	0.13
2	.22	9.4	− 76.5	+179.8	.21
3	.21	10.1	− 65.8	− 28.2	.15
4	.30	8.8	− 20.8	+ 42.8	.18
5	.24	9.4	+ 55.3	−247.5	.11
6	.27	9.4	+369.4	+130.3	.22
Nova	.37 to .12	8 to 12	+ 23.46	+ 32.17	

stars. The second column gives the mean diameter of the star images in fractions of a millimeter. The image of the *Nova* diminished from 0.37 mm to 0.12 mm in the eleven months of observation, although the exposure time was increased from 5 to 15 minutes as the *Nova* decreased from somewhat brighter than the 8th down to the 12th magnitude.

Columns 4 and 5 show the co-ordinates of the comparison stars, in units of the scale of the measuring machine, referred to the mean of all. Thus in longitude four are below and two above the mean, while in latitude three are above and three below. The last column gives what Professor Schlesinger has well called the "Dependence" of each comparison star. The figures indicate the relative influence of each star upon the parallax to be found.

In the case of this star it was decided to measure the parallactic displacement parallel to the ecliptic rather than that parallel to the equator.

The usual method of reduction was followed. One plate was selected as a standard, in this case No. 446, and all of the others referred to this standard by a linear relation of the form

$$ax+by+c=x-x_0.$$

After a least square reduction for the a, b, and c for each plate, the residuals for the parallax star were found. These residuals involve the displacements due to parallax and proper motion plus a constant. Each plate then gives an equation of the form

$$P\pi+t\mu+c=m$$

from which the parallax and proper motion may be obtained.

TABLE III

REDUCTIONS FOR *Nova Lacertae*

Plate	Solution (m)	Weight (p)	Longitude Parallax Factor (P)	R.A. Parallax Factor	Time in Days (t)	Residual (v)	Residual in Arc
359........	0.000	0.8	−0.983	−0.765	−189	0.000	0ʺ000
360........	+ .002	1.3	− .978	− .697	−182	− .001	− .003
432........	+ .024	1.0	+ .984	+ .889	− 21	− .001	− .003
438........	+ .047	.5	+1.016	+ .792	− 7	− .022	− .059
446........	.000	1.0	+1.012	+ .727	0	+ .025	+ .066
498........	+ .036	1.0	+ .299	− .268	+ 70	− .009	− .024
531........	+ .046	1.0	− .176	− .648	+ 98	− .019	− .051
536........	+ .036	1.0	− .405	− .785	+112	− .009	− .024
548........	+ .001	1.0	− .736	− .911	+136	+ .026	+ .069
563........	+ .029	1.0	− .838	− .916	+146	− .001	− .003

NORMAL EQUATIONS

$$6.052\pi+\ .9160\mu-1.4093c=+.0081$$
$$13.8942\mu-1.4990c=+.1436$$
$$+9.600\ c=+.1975$$

$$c=+.0200$$
$$\mu=+.0079=+0''021$$
$$\pi=+.0048\ \pm\ .0051=+\mathbf{0''013}\pm\mathbf{0''014}$$

Probable error corresponding to unit weight, $\pm .0123=\pm 0''033$.

In Table III, column 2 gives the residual for the *Nova* of each plate.

Column 3 gives the weight depending upon the number of exposures and the quality of the images.

Column 4 the parallax factors in longitude: two minus, four plus, and four minus.

Just for comparison the parallax factors in right ascension are given. For all except a few plates these run smaller than the corresponding factors in longitude.

The sixth column shows the time in days, reckoned backward and forward from the standard plate No. 446.

From these values ten equations are formed which yield the normal equations shown in Table III. The value of c is of no especial interest; μ is the proper motion in longitude for 100 days.

The relative parallax comes out **+0."013** ±0."014 with a probable error from one plate of unit weight of ±0."033. According to Kapteyn's table of average parallaxes, that of the comparison stars may be taken as 0."005, which would make the absolute parallax of the *Nova* +0."018.

The above parallax is less accurate than might be expected from a normal star for two reasons: first, because it was necessary to take the first two plates with the telescope east; second, because the *Nova* diminished four magnitudes during the eleven months, thus rendering the possibility of influence from guiding error considerably greater. If the *Nova* is still bright enough, additional plates will be taken next summer, and a new reduction made, omitting the first two plates and using much fainter comparison stars.

So far as I have been able to learn parallax determinations of but two other novae have been made:

For *Nova Andromedae* (1885) Franz found, from heliometer observations, a parallax of −0."32 ±0."12.

Several determinations of the parallax of *Nova Persei* (1901) were made:

Hartwig found	+0."16 ±0."06
Chase	−0."012±0."035
Greenwich observers	<0."1
Bergstrand	+0."029±0."008

The first two determinations were made from heliometer observations, the last two by photography.

Without claiming too much accuracy for my result, we are safe in saying that the parallax of *Nova Lacertae* is exceedingly small.

If the value given above were correct, it would mean that the outburst observed in 1910 really occurred 180 years ago.

YERKES OBSERVATORY
January 10, 1912

ON THE ORIGIN OF SELECTIVE CONTINUOUS ABSORPTION OF BAND AND SERIES SPECTRA

By J. KOENIGSBERGER

I wish here to enumerate briefly a few experiments which Mr. Küpfer and I have previously described at greater length. I am indebted to the Board of Trustees of the Elizabeth Thompson Science Fund of Boston for their assistance in these experiments. A great number of elements and compounds were heated in very highly evacuated flasks of soft and hard glass or of quartz. The absorption of the vapor was then examined. A good Rowland grating in the third order dispersed the transmitted light, making it possible to see what substances gave a band spectrum resolvable into lines, and what ones a selective continuous absorption. For the latter, $p\frac{e}{m}$ could be reckoned by the formula given elsewhere, and in the same way p represents the number of the vibrating parts of the molecule, and $\frac{e}{m}$ the ratio of electricity to mass.

The following fact was then observed: all colored vapors which possess no reversible chemical dissociation, i.e., a steady association

Substance	Absorption	Chemical Dissociation	Substance	Absorption	Chemical Dissociation
Indigo red	*S*	−	Iodine trichloride	*B*	+
Indigo blue	*S*	−	Selenium chloride	*B*	+
Alizarine	*S*	−	Arsenic tri-iodide	*B*	?
Anthrachinon derivatives	*S*	−	Tellurium dichloride	*B*	+
Iodeosin	*S*	−	Tellurium dibromide	*B*	+
Iron chloride	*S*	−	Selenic acid	*B*	?
Chromium chloride	*S*	−	Manganese superchloride	*B*	+
Tin iodide	*S*	−	Chromium oxychloride	*B*	?
Nickel chloride	*S*	−	Chlorine dioxide	*B*	+
Iodine	*B*	+	Chlorine monoxide	*B*	+
Bromine	*B*	+	Nitrogen dioxide	*B*	+
Chlorine	*B*	+			
Sulphur	*B*	+			
Selenium	*B*	+			

S = Selective continuous absorption. *B* = band absorption. + = reversible chemical dissociation either at the temperature of the experiment, or at a somewhat higher temperature. ? = doubtful, as no observations on dissociation were made.

and dissociation, give a continuous selective absorption. All substances which, on the other hand, have band spectrum resolvable into lines, dissociate chemically either at a temperature at which they give the band spectrum, or at a higher temperature.

The metal vapors of zinc, cadmium, tin, arsenic, lead, mercury, and thallium show no absorption at 550°, neither continuous nor discontinuous, in the visible spectrum. The metals were placed in highly evacuated glass flasks entirely free from gas, and the flasks then closed. In this way all chemical reaction was out of the question. The metalloids, which, like sulphur, selenium, tellurium, are all polyatomic, show at a lower temperature a continuous, at a higher a band, absorption. Chlorine, bromine, iodine give a band spectrum in conjunction with a continuous selective absorption.

This behavior of compounds and the metalloids seems to prove the dependence of the band absorption upon processes which effect chemical dissociation, either with dissociation of the molecule or with its reunion. The experiments of E. J. Evans[1] on bromine and of J. I. Graham[2] on sulphur show that bromine Br_2, as well as S_8 and S_2 in these cases must be present if there is band absorption.

Therefore, the process of the dissociation of Br_2 into $2Br$, of S_8 into $4S_2$, and of S_2 into $2S$, or the union of $2Br$ into Br_2, as of $4S_2$ into S_8 points to the probability of vibrations in the bands. Which of these is the real cause we cannot at present say. Below we note that for line emission of the series-spectrum a similar hypothesis is possible. If substances show a band spectrum then polyatomic molecules must be present, which also holds for metalloids. We rather think that sodium vapor, which, as R. W. Wood has shown, has band absorption, is not strongly monatomic but possesses a complex molecule at a temperature at which it is ionized. Experiments on the mobility of the ions prove also the existence of such complex molecules, which, therefore, depends on the formation of the ions. On the other hand, for the same reason, it should not be possible to observe a band spectrum for the inert gases.

It is often not possible to prove chemical dissociation at temperatures at which band spectrum occurs, as in the case of bromine

[1] *Astrophysical Journal*, **32**, 291, 1910.

[2] *Proc. Roy. Soc.*, A, **84**, 311, 1910.

and iodine; but the band spectrum is weaker, the higher the temperature of the chemical dissociation. Then the process of dissociation and reunion takes place probably in immediate succession without the atoms *Br* or *I* existing as such free for any appreciable length of time. On the other hand, if the gas has become monatomic, a band spectrum is either not visible or only very seldom. The band spectrum which benzol, ammonia vapor, etc., shows in the ultra-violet, would have its cause in the rupture and the reunion of one or more chemical bonds. What bonds these may be, is a chemical question, and one which the author cannot answer.

The band absorption occurs on the boundaries of the continuous selective absorption and indeed chiefly on the side of the greater wave-lengths. The greater the density so much the more is the intensity of the band absorption attenuated and shifted, because the continuous absorption is increased. The cause of this is not due to the darkness caused by the absorption, which would prevent the band absorption from being seen, for we can prove that there is no band absorption if we diminish the thickness of the vapor layer and increase the density. On the other hand, by diminution of the density the continuous absorption becomes weakened and compressed. Therefore, if we wish to have a sufficiently intense band absorption, we must increase the density of the vapor layer; in this case the band absorption takes up the greater part of the spectrum. Band spectrum occurs by the rupture of those chemical bonds which give also continuous selective absorption. Whether the influence of the density on the band spectrum depends on the exterior collisions[1] or on a change of the dissociation process, it is not now possible to say. It appears as if the conditions in which the band spectrum is absorbed and emitted remain relatively longer in the case of smaller density.

Gases which dissociate with difficulty, as nitrogen and oxygen, show a band spectrum at a correspondingly higher temperature.

The continuous selective absorption in the visible part of the

[1] The strong damping of vibration which produces the continuous selective absorption is not caused by collision with other gaseous molecules. We could prove this by observations on the vapors of undissociated colored substances. In these substances the damping of vibrations is quite independent of the density, for it remains the same if the density is 1/10000 of the original.

spectrum results only through vibrating electrons so far as it has been possible to calculate. *Continuous selective absorption is due to the normal state of gas molecules; the band spectrum absorption and emission only to an intermediary non-stationary condition.*

The series spectra appear on the other hand to have an entirely different cause, viz., electric dissociation or ionization. We conclude that this is so from the following observations on canal rays when we take the hydrogen atom as the simplest example.

The canal rays emit light, as we have further found, only when they come into a space filled with gas; in high vacuum ($1 . 10^{-5}$ mm) no light can be seen, and, indeed, is so much the weaker, the higher the vacuum. Hence the light-emission is brought about by a process between the moving canal-ray particles and the gaseous molecules which are at rest. As W. Wien and J. J. Thomson have found, the canal rays change their charge and become absorbed. We have found that the change of charge gives no appreciable loss of velocity. The absorption is brought about through a diminution of the number of the canal-ray particles by diffusion and total or partial stopping. Emitted light which shows the Doppler effect, as discovered by J. Stark, cannot result from particles at rest, or partially stopped, but only from those moving in the direction of the canal rays with the original velocity. Hence the emission of light can occur not by absorption but only by the change of charge. This change of charge is of two kinds: (1) neutralization of the positive part by attraction of an electron, (2) dissociation of the neutral part into positive, by giving up of an electron.

It is evident that by magnetic deviation only the neutral ray emits light. The positive will not emit light so long as it becomes neutral again, through a partial change of charge. The experiment succeeds best at a pressure of about $5 . 10^{-3}$ to $4 . 10^{-2}$ mm. At a greater pressure the change of charge takes place too quickly, and the positive ray becomes neutral too quickly; on the other hand, at a smaller pressure the change of charge occurs too seldom and the light emission is too weak. Hence the dissociation of neutral into positive causes the light emission. The positive ion *in statu nascendi* emits light in the visible part of the series spectrum. This opinion agrees to a certain extent with that of J. Stark.

The distance, l, on which the newly formed ion emits light and therefore the length of time of the light emission is very short; l, according to our observations, is smaller than 0.5 cm, therefore $t=\frac{l}{v}2.5\cdot 10^{-10}$ sec. But this time corresponds to a large number of wave-lengths. We cannot say whether the emission is stopped after this time or whether the condition ceases in which the series spectrum is emitted and absorbed. The number of canal-ray particles is so small that an absorption of the light through them cannot be proved, as W. Wien has shown. The change of charge from positive to neutral brings about no light emission, as the experiments show.

FREIBURG IN BADEN
January 16, 1912

Minor Contributions and Notes

THE ASTRONOMICAL AND ASTROPHYSICAL SOCIETY

The thirteenth meeting of the Astronomical and Astrophysical Society of America was held in the auditorium of the Carnegie Institution of Washington on December 27, 28, and 29, 1911, in connection with the meeting of the American Association for the Advancement of Science. There were four sessions of the Society, presided over by Professor E. C. Pickering, president. Sixty-four members of the Society were in attendance. The program included the following papers:

E. W. Brown: "A Device for Facilitating Various Forms of Computation."
H. S. Davis: "The *Astronomischer Jahresbericht;* an Announcement."
H. S. Davis: "The Lesson of Joseph Piazzi's Life."
Joel Stebbins: "The Variability of *Polaris.*"
J. A. Parkhurst: "Magnitudes, Colors and Spectra of Standard Stars within Seventeen Degrees of the North Pole" (Lantern).
J. G. Porter: "A Comparison of Doctor Peters' Celestial Charts with the Photographic Charts of the Sky."
G. H. Peters: "The New Twin Photographic Telescope of the United States Naval Observatory" (Lantern).
Miss S. F. Whiting: "The Use of Special Topics in Teaching Astronomy."
J. C. Duncan: "The Orbit of the Spectroscopic Binary *β Scorpii.*"
Frederick Slocum: "The Dissolution of Solar Prominences" (Lantern).
Frederick Slocum: "The Parallax of *Nova Lacertae* (1910)."
W. J. Humphreys: "A Simple Pyrheliometer."
F. W. Very: "The Violle Actinometer as an Instrument of Precision."
Miss A. J. Cannon: "The Revised Draper Catalogue."
H. N. Russell: "Notes on the Calculation of the Elements of *Algol* Variables."
H. N. Russell: "The Eclipsing Variables, W *Centauri* and W *Ursae Majoris.*"
J. S. Plaskett: "The Solar Rotation" (Lantern).
F. E. Ross: "The Moon's Mean Parallax."
Eric Doolittle: "The Secular Variations of the Elements of the Orbits of the Four Inner Planets."
C. F. Talman (introduced by W. J. Humphreys): "The Language of Meteorology."

F. H. Loud: "May Astronomy Derive Any Benefit from the Dissemination of Esperanto?"
W. S. Eichelberger and H. R. Morgan: "On the Flexure of a Meridian Circle."
E. S. King: "Tests with Standard Electric Lamps."
J. A. Brashear: "Recent Interviews with Optical Glass Manufacturers of France and Germany."
E. E. Barnard: "Some Observations with the 60-Inch Reflecting Telescope of the Mt. Wilson Solar Observatory" (Lantern).
E. E. Barnard: "Photographic Observations of Brooks' Comet of 1911" (Lantern).
F. B. Littell: "Personal Equation Apparatus of the 9-inch Transit Circle of the Naval Observatory."
Asaph Hall: "Observations of the Satellites of *Uranus* and *Neptune* Made at the Naval Observatory, 1908–1910."
W. S. Eichelberger: "Paris Conference of October, 1911."
Zaccheus Daniel and F. Schlesinger: "The Spectrum and Orbit of β *Scorpii*."

Reports were presented by the Committees on Comets and on Photographic Astrometry, and from the Committee on Co-operation in the Teaching of Astronomy.

A new Committee on Asteroids was formed, consisting of E. W. Brown, J. H. Metcalf, G. H. Peters, and A. O. Leuschner.

There were also two sessions of Section A of the American Association, presided over by Edwin B. Frost, vice-president of the Section. At these sessions were presented the address of the retiring vice-president of the section, Professor E. H. Moore, "On the Foundations of the Theory of Linear Integral Equations"; and papers by Lewis Boss on "Recent Researches as to the Systematic Motions of the Stars," and by J. H. Metcalf on "The Asteroid Problem."

The next meeting of the Astronomical and Astrophysical Society will be held at the Allegheny Observatory in the coming summer.

F.

Reviews

The Sun. By Charles G. Abbot. New York and London: D. Appleton & Co., 1911. 8vo, pp. xxv+448, with 26 plates, 72 illustrations in the text, and 34 tables. $2.50.

This is the fourth book on the sun that has appeared within the past fifteen months, the other three being *Les théories modernes du soleil*, by J. Bosler, *Vorlesungen über die Physik der Sonne*, by E. Pringsheim, and Stratonoff's *The Sun*, printed in Russian.

This new book by Abbot is the first on this subject to be printed in English since the last edition of *The Sun* by Young in 1895. For fifteen years Young's book was the authority upon all matters pertaining to the sun. That interval was marked by great progress in solar investigation and revised editions of the book appeared at short intervals. Since 1895 there has been an even greater advance. As Abbot says in his preface:

> Within the last fifteen years we have seen the publication of Rowland's great table of solar spectrum wave-lengths, the establishment of the Yerkes, Kodaikanal, Mount Wilson, and other observatories largely devoted to solar researches, the photography of the spectrum of the corona and of the chromosphere at total solar eclipses, Hale's brilliant discovery of magnetic fields in sun-spots, the determination of the rotation periods of the sun at different levels, as well as at all solar latitudes, Langley's bolometric investigations of the sun's infra-red spectrum, and the recent Smithsonian determinations of the absolute intensity of the solar radiation outside our atmosphere. The great interest in such researches has been marked by the establishment of the International Solar Union, and its enthusiastic gatherings of the foremost investigators from all lands.
>
> The time seems ripe for collecting the splendid array of new solar knowledge which such unprecedented activity has produced, and for discussing the probable nature of the sun in the light gained.

Young's book is now out of print and it is the intention of the author that this book shall take its place. In fact, some of the illustrations and text of Young's book have been incorporated into the present volume, issued by the same publishers.

An idea of the general scope of the work may be obtained from the chapter headings: i, "The Solar System"; ii, "Instruments and Methods

Used in Solar Investigation"; iii, "The Photosphere"; iv, "Eclipses and the Outer Solar Envelopes"; v, "Sun-Spots, Faculae, and Granulation"; vi, "What Is the Sun?" vii, "The Sun as the Earth's Source of Heat"; viii, "The Sun's Influence on Plant Life"; ix, "Utilizing Solar Energy"; x, "The Sun among the Stars."

As will be seen, the sun is considered in three aspects: first, as the controlling member of the solar system; second, as a star, interesting in itself, and typical of a large class of stars; third, as the source of light and heat, and through them of life on the earth.

Thus a wide field is covered. In addition to the matter pertaining directly to the sun and to astrophysics, many facts and theories are given which will prove of interest to the meteorologist, geologist, botanist, and engineer. And even the student of household economics will find here something of interest in the section on solar cooking appliances.

To attempt to cover such a wide field in a single volume of this size is rather a bold undertaking, but Mr. Abbot has, in general, succeeded in treating each subject simply, clearly, and concisely. A few exceptions may be noted. Fig. 58 on p. 260 and the accompanying explanation are, to me, obscure. Again, on p. 53, the reason for inserting a prism into a plane grating spectroscope is not made clear. On p. 121, referring to solar prominences, we read: "And now the spectroheliograph has enabled us to recognize them frequently as dark hydrogen flocculi on the disk itself. A view of the sun through the $H\alpha$ (C) line is best adapted for this purpose. . . . ." The fact that the prominences can, with sufficiently high dispersion, be equally well shown as dark *calcium* flocculi seems here to be ignored.

In the chapter entitled "What Is the Sun?" are given outlines of the views of Young, Halm, Schmidt, Julius, and Abbot. These views contain the most recent theories of the causes of solar phenomena, so far as such theories can be stated in simple, non-mathematical language.

The strongest parts of the book are naturally those dealing with bolometry, pyrheliometry, and the theories of radiation—subjects to which Mr. Abbot has for years devoted special study. But in trying to do full justice to these subjects he has been forced to treat others somewhat *stiefmütterlich*, as a German might say. For example, we find numerous figures of pyrheliometers but no illustration of a spectroheliograph. And the same set of bolographs is given on p. 83 and on p. 292.

The illustrations are, with one or two exceptions, very good. I am glad to see Langley's drawing of the sun-spot of 1873 used as a frontispiece. During the past thirty years this picture has been reproduced in

scores of astronomical books, and I trust that it will continue to be reproduced, at least until someone makes a better one. Photography has done wonders for the advancement of solar research, as is well shown by the many reproductions of photographs in this book, but it has not as yet superseded direct visual observations for the delineation of the details of sun-spots. Careful and persistent visual study of sun-spots is as important today as it ever was. This drawing should be a perpetual source of inspiration to amateurs and visual observers of the sun.

The book as a whole will be a welcome addition to the literature of the subject, and it is to be hoped that the author will follow Young's example and try to keep it up to date by new editions whenever necessary. The rapidity of the progress in this field is shown by the fact that some of the material in the book has already been superseded by results based upon later and better observations. This is true in regard to Table IX, p. 126, and Table XXXIV, p. 430.

The addition of many more references to original sources would, in my opinion, greatly increase the value of the book.

FREDERICK SLOCUM

Cours d'astronomie, Première partie: Astronomie théorique. Deuxième édition entièrement refondue. Par H. ANDOYER. Paris: A. Hermann, 1911. Pp. 375, with 83 figures. Fr. 12.

The revised edition of the first volume of M. Andoyer's work follows the plan of the first edition, which was reviewed four years ago in this Journal (**25**, 288, 1907). Even to a larger extent than in the first edition, all the theoretical discussions are based upon a small number of fundamental theories, which are completely developed. Thus we find in chap. i an excellent development of spherical trigonometry and in chap. iii a very full discussion of change of co-ordinates. The work now possesses, both in its composition and also in its printing, the scholarly finish and excellence which are characteristic of French scientific books.

F. R. MOULTON